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EDITED BY

CHARLES S. MYERS

WITH THE ASSISTANCE OF

F. C. BARTLETT, S. S. BRIERLEY, AND C. BURT

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THE BRITISH JOURNAL OF PSYCHOLOGY GENERAL SECTION

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MIND AND MEDIUM IN ART[1]. (I.)

BY CHARLES MARRIOTT.

I.

THE following notes are prompted by the belief that in all aesthetic discussion, and psychological experiment in connexion with the subject of aesthetics, too little attention is paid to the question of the direct action on the mind of the medium itself. By 'medium' is understood the tools and materials employed in the particular art concerned.

A few simple illustrations may make this clearer. Let us take a symbol with which everybody is familiar, particularly since its mournful application in the late war: the cross. As carried out in wood, that is to say in the form of two crossed sticks or planks, apart from any religious meaning that it conveys most people find the symbol aesthetically satisfactory if the proportions are reasonably good. But, carried out in stone, in the same form and proportions, the symbol becomes unsatisfactory. It seems to need modification. In practice the modification is generally made. The most popular forms of the cross in stone are either the Celtic, or 'wheel,' cross, or else the tall form, common in village crosses, in which the shaft is greatly elongated and generally tapering from the base, and the arms are reduced to comparative insignificance.

The reason seems to me quite simple. In the wooden form, whether the two sticks or planks are mortised or merely nailed together, the grain of the wood, though it may not be perceptible to the eye, is felt to run horizontally across the arms. The wooden cross explains itself as a structure. Repeated in stone the same form does not explain itself. Since it is inconceivable that the grain of the stone can run vertically in the shaft and horizontally in the arms, the impression given is that the cross has been punched out of the stone in an arbitrary manner in disregard of its nature and substance. Not only that, but the impression conveyed is that of actual weakness at the crossing of the shaft and arms. In addition there is the sense of difficulty in workmanship; that it must have cost an amount of labour disproportionate to the result achieved to cut the stone to the lateral depth necessary for the projection of the arms to that extent. As I said, in practice the feeling of discrepancy with regard to the grain, the effect of weakness at the crossing, and the sense of disproportionate labour are all generally avoided by certain modifications. In the Celtic cross an amount of stone is left, in the shape of the 'wheel,' which not only strengthens the structure but, so to speak, contradicts the method of construction employed in wood, and actually lessens the labour; and in the tall cross the same results are obtained by evasion; by making much of the shaft and little of the arms. When and why these modifications were adopted in translating the wooden cross into stone it is impossible to say; but I am persuaded that they were prompted by aesthetic instinct having regard to the nature of stone. If it be objected that the reason was practical, my answer is that practical and aesthetic reasons are at bottom the same thing; or, to put it another way, that aesthetic appreciation is dependent upon the sense, which may not be conscious, of practical problems effectively solved. However, for the moment, the important point is that here we have a simple form and proportion which is satisfactory in one material and unsatisfactory in another.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

Now let us take an illustration from architecture. It would be quite possible, I suppose, to build a Parthenon of the same scale and proportion in brick; but I do not think that the result would be satisfactory. Whether or not Doric architecture originated in the imitation of wooden structures, it is obviously a style adapted to construction with stones which share with timber the character of length. Whatever it is or is not, classical architecture is essentially a style of building with long stones. For satisfactory translation into brick it involves the modifications implied in Renaissance, represented in England by Tudor and Queen Anne. Here again we have a form which, for aesthetic reasons based upon practical convenience, is satisfactory in one material and unsatisfactory in another.

Or, again, we might take an illustration from the printed page, and one which carries the argument a little further because it transfers the attention from the materials used to the act of using them. Nobody can look at an illuminated manuscript without feeling that the character employed is well adapted to the act of writing. You can see the formation of the letters with the pen or brush. With the introduction of wood-engraving, which is the link between writing and printing, this fitness of the character becomes less evident. You can still follow in sympathy the movements of the engraver's hand in cutting the type, but the form of the character seems too elaborate for the method. With the introduction of printing from movable type, or typography proper, the unfitness of the character for casting positively jumps at you; and, in fact, with the advance of printing the Gothic character was generally abandoned in favour of Roman, which was probably an engraved or incised character before it was a written one.

Thus, in the transition from writing to printing, the form of the character was modified and even changed out of regard for the materials and the convenience of the hand in using them. The same symbols were employed, and with exactly the same meaning; but they were adapted to the changed methods in book production. Not only that, but our aesthetic pleasure in the written or printed page depends a good deal on this fact. Minor variations in form and irregularities in arrangement which add beauty and interest to the written page, because they suggest the movement and convenience of the hand, have no meaning in the printed page. Instead of an aesthetic merit they become an aesthetic defect; though the purpose of the text, to convey meaning, remains exactly the same. Apart from the strain it puts upon the eyes, the Gothic type of a German newspaper offends the aesthetic sense because it looks ill-adapted to the methods of printing as it was not to formation with the pen or brush.

The above illustrations have the advantage of not being complicated with any question of likeness to nature. They show, or seem to show, that even in the more abstract conditions of art we cannot consider form apart from the means by which it is produced. When we turn to the arts of representation we find exactly the same thing. For our full aesthetic satisfaction, any form of nature must be adapted to the material in which it is represented and to the convenience of the hand in using that material. Thus, we expect a broader treatment of landscape in water-colour than in oils because the, so to speak, 'natural' play of water-colour is in broad washes. Or, to take extreme illustrations, stained-glass, mosaic, tapestry and embroidery obviously do not lend themselves with ease to such close imitation of nature as either oil or water-colours. The same natural forms may be represented in all these materials; but in each case there are modifications determined by the convenient use of the material concerned. In a note to his Lectures on Architecture and Painting delivered at Edinburgh, November, 1853, Ruskin put this commonplace of art, as it might be called, into the following words: "In every branch of art, only so much imitation of nature is to be admitted as is consistent with the ease of the workman and the capacities of the material."

II.

The question, from a psychological point of view, is how far our enjoyment of a work of art is dependent upon this adaptation of means to end; whether, in fact, Ruskin's axiom is an arbitrary aesthetic rule or a commonplace of human psychology. Personally, as I have tried to show, I believe that the dependence is much greater and much closer than is commonly supposed, and that in itself it affords the most promising guide to both artistic education and psychological experiment.

The objection that we can enjoy works of art without knowing how they are done, and that in moments of highest aesthetic enjoyment we become unconscious of the means, is only apparent. Directly the means are used out of character we *do* become conscious of them, whether we understand their characteristic use or not; as is implied in the old saying *ars est celare artem.*

Nor does unconsciousness of the means seem to depend on likeness to nature in the sense of imitation—as the old saying is commonly interpreted. A very wide departure from nature is accepted by the simplest intelligence so long as the means are used in character. Leaving out such crude examples as the child's wooden horse or Dutch doll, the outline drawing is a case in point. It does not really resemble what it represents, but it presents no difficulty to the mind because it explains itself by the means adopted: the pointed pencil. The same is true of every form of art in which the means are obvious; such as stained-glass or mosaic. Paradoxical as it may sound, the means are ignored in proportion as they are obvious. It is when the means are of a kind to allow the artist to "play the game of art closely up to the real aspects of nature," as in painting, that difficulties begin and the true meaning of *ars est celare artem* is confused with the question of likeness to nature in the sense of imitation. The same person who will swallow an Egyptian papyrus will jib at an Impressionist painting. He says it looks 'painty.' The reason seems to me quite simple. In the papyrus the means though perfectly obvious are ignored because they are used in character; in the Impressionist painting they are obtruded because, to a certain extent, they are used out of character. To a certain extent they are used to create optical illusion; they do not seem entirely "consistent with the ease of the workman and the capacities of the material."

This habit of the human mind of straining at gnats and swallowing camels in the use of materials seems to me to throw light upon the very nature of art and of artistic appreciation. Broadly speaking there are two views of art as commonly held: that it is primarily the representation of nature, and that it is primarily the direct action of abstract form, colour and proportion or, in general terms, of design. Both views assume the transference of emotion as the aim of art, and each allows for the other as an aid to that purpose; but neither takes much account of the direct action of the medium in the process.

The suggestion that I would put forward is that art is primarily the characteristic use of materials, and that the representation of nature and the direct action of design are both incidental to that. They may or may not be present in the particular art concerned. So far from denying either, this view provides the connecting link or the reconciling factor between them. In this view the transference of emotion is looked upon as a consequence rather than an aim, and aesthetic appreciation as mainly a sympathetic response, not necessarily conscious, to the characteristic use of tools and materials.

That this view ignores any distinction between art and craft may be cheerfully admitted. The distinction may exist, but most of the troubles of art, including the separation of art from life, to the disadvantage of both, are due to the various attempts to fix it arbitrarily. For our purpose it is enough to say that craft is a necessary condition of art, and that the nature of art and of aesthetic appreciation can be studied profitably only in that condition. I have the tantalising conviction that the whole aesthetic problem is stated in the Athanasian Creed—if we could only work it out! At any rate we can say that art is neither made nor begotten but proceeding from craftsmanship in relation to the subject or idea. Or, to put it another way, the relation between art and craft may be not unlike that suggested by William James, in *Human Immortality*, as existing between thought and the brain. That is to say, that the function of craft is not creative but transmissive or, if you like to put it that way, limiting. This would account for the fact that there are plenty of people with a high degree of aesthetic sensibility, and even 'creative ideas,' but no sign of artistic talent. They lack the limiting capacity of craftsmanship which is the necessary condition of art. It would also account for the fact that though art is universal its limitation through craftsmanship results in the expression of what we call personality.

III.

Let us put it this way: all art is the record of human gestures in the presence of the subject or idea and as conditioned by the nature of the medium in which they are made. This would include even literature, which may be regarded as a gesture of the mind in the medium of words. It is notorious that we can enjoy hearing poetry in a language that we do not understand; indeed, I am not sure that *pure* aesthetic appreciation of poetry does not exclude the dictionary meaning of words—as pure aesthetic enjoyment of painting excludes the literal meaning of the subject. Aesthetic appreciation is the recognition, which may be instinctive and may be informed, of the propriety or 'naturalness' of such gestures having regard to the nature of the subject or idea and of the tools and materials with and in which they are made. With a responsive material, such as oil paint, these gestures may be as closely imitative of the subject as is consistent with freedom of brush-work—Ruskin's "ease of the workman"; but with a refractory material, such as granite, they must be far from it in order to seem natural.

In his book on *Art* (Chatto and Windus, 1914) Mr Clive Bell has aptly defined the quality that moves us common to all works of visual art as "significant form." But significant of what? Of human gestures in the medium concerned. As I tried to show in the illustrations of the cross, the Doric building, and the characters used in writing and printing, a form which may be significant in one material ceases to be so in another when the human gestures implied are obviously inconvenient. Oddly enough, the pictures in Mr Bell's book are all striking illustrations of the way "significant form" is conditioned by "the ease of the workman and the capacities of the material." Each represents a different medium, and in each of them the gestures implied are expressive not only of the subject but of the tools and materials employed.

Incidentally, this conception of "significant form" would explain the difference between the emotions we receive from nature directly and from works of art and, also, the occasional difficulty in distinguishing between the two. If we consider the scenes or aspects of nature which excite emotions which may be called aesthetic, we shall find, I think, that they are generally such as recall human gestures or, rather, the ampler gestures of a Creator working in the materials at His command. This might suggest the ultimate meaning of art. At any rate, there is some very sound aesthetics in 'Omnia Opera.'

The validity of the view put forward in this paper could only be tested by extensive and careful experiment with a number of subjects. I merely suggest the direction in which they should be made; that is to say, with abstract forms, may be, but always in relation to human and not mechanical gesture in some definite material. In a paper intended for discussion assertion is perhaps better than argument; and I shall content myself, now, with suggesting some of the consequences and, as I think, advantages of the view put forward.

1. It abolishes all the distinctions between one art and another on the grounds of the representation or the non-representation of nature.

2. It abolishes all the artificial and uncertain distinctions between various forms of the same art; such as decorative or pictorial painting, realistic or romantic poetry or drama.

3. At the same time it makes the sharpest possible distinction between one art and another. Whatever painting is or is not it involves the use of paint; whatever sculpture is or is not it involves the use of some substance that can be carved or moulded; whatever architecture is or is not it involves the use of building materials; whatever music is or is not it involves the use of musical sounds and of instruments to produce them; and whatever literature is or is not it involves the use of words. Only on the basis of the tools and materials used in them can confusion between the arts be avoided. On the other hand, it is when the arts are divided on a craft basis that they are most harmoniously combined—as we see in the Greek temple or the Gothic cathedral.

4. Without attempting to define the purpose of art it will include every purpose for which any art could possibly be practised; from the mere exercise of the muscles to the illustration of a theme or the interpretation of an idea.

5. While allowing for all the infinite degrees of art it brings all practitioners of the same art into the same category; the house-painter with Michelangelo, the letter-writer with Shakespeare. The differences between elementary and advanced, good, bad and indifferent art remain as before, though with a much firmer guide to their establishment, but at no point in the practice of any particular art can you say: "This is art" or "This is not art." Thus, if a man paints a door, I do not see how flatness can be regarded as other than an aesthetic consideration; though, of course, an extremely simple one.

6. It brings art into close relations with all other forms of human activity in work and play.

From the human point of view the last two advantages are by far the most important. So long as art is regarded as primarily the representation of nature, or as primarily the direct action of design, it is bound to be regarded as a special subject. The difficulty is to get the ordinary person to carry into aesthetic regions the perception and judgment that he displays in the other departments of life. He will persist in thinking of art as a mystery complicated by arbitrary rules of taste; and the consequence is that he chooses his clothes and pursues his hobbies on sound aesthetic principles, so far as they go, and takes his art and literature on trust. It is a fact that the pictures in an average home are as a rule less good artistically than the carpets and curtains, and the domestic reading is less good literature than the letters and conversation. I am not proposing anything new—indeed it is the oldest view of art—but only that the subject of art and literature should be approached from a fresh angle. Once accustom a man to the idea that art is primarily the characteristic use of tools and materials in view of the particular purpose, and you have given him a clue to the highest aesthetic appreciation. Whatever art may be, essentially, in the last analysis all aesthetic virtues are virtues of necessity. The laws of painting are as surely derived from the nature of paint and brushes as the laws of cricket are derived from the nature of bat and ball. Any man who has handled a tool in a hobby or an implement in a game is on the way to understanding art in its highest developments. The purpose may be different, but the principles are the same. The advantages of this approach to the subject do not, however, end with aesthetic appreciation. They reflect backwards upon work and play. The first-hand knowledge and judgment derived from work, of whatever kind, are carried into the aesthetic region, and the refinements of art are instinctively applied to work; so that sweeping a floor becomes an artistic exercise.

All art is, finally, nature, but nature in the larger sense defined by Reynolds in his *Seventh Discourse*. "My notion of nature comprehends not only the forms which nature produces, but also the nature and internal fabric and organisation, as I may call it, of the human mind and imagination." If to these words we add "and the nature of the tools and materials used by man," it seems to me that we have a conception of art that will cover all its practices and, at any rate potentially, explain all its phenomena.

MIND AND MEDIUM IN ART[1]. (II.)

BY A. B. WALKLEY.

MR MARRIOTT opens his enquiry by approaching art from without, and the close of it finds him, I think, left still outside. He takes the 'practical' view of aesthetics, as distinguished from the 'theoretic,' which, in its innumerable varieties, has been the view normally taken throughout the history of the science, from its Greek beginnings to the present day. Indeed, he takes the ultra-practical or 'craftsman's' view. "Craft," he says, "is a necessary condition of art, and the nature of art and of aesthetic appreciation can be studied profitably only in that condition." This is an extreme position. Mr Marriott will not take it as a reproach, but, from his own statement, as a compliment, if I call it, as Greek criticism would have called it, 'banausic.'

He starts with the familiar fact that construction is modified by materials. A stone cross naturally takes a different shape from a wooden cross. The forms of classical architecture were conditioned by the use of long stones. In the transition from writing to printing script changed as the practical conditions changed. Conclusion: "practical and aesthetic reasons are at bottom the same thing"—or "aesthetic appreciation is dependent upon the sense, which may not be conscious, of practical problems effectively solved." I fail to see how this conclusion logically follows from these premises. Because practical reasons have not been ignored in the aesthetic field, does it necessarily follow that there are no others? That they are "the same thing" as aesthetic reasons? Is conformity to the nature of the material the sole criterion of beauty? "Aesthetic appreciation is dependent upon the sense, etc." Is this true; or should we prefer to say "*includes* the sense"?

But Mr Marriott is firm about it. "Art is primarily the characteristic use of materials." Imagination (I use the word to cover both his "representation" and "design") is merely incidental—and may not be there at all. Tools and materials are 'the thing.' "The limitation of art through craftsmanship results in the expression of what we call personality." Some people would call this putting the cart before the horse. They would say that the expression of personality, in the process of externalisation, has to use craftsmanship. I will return to that later. But we are already a long way from the notion of art as a spiritual activity, as an operation of the mind.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

And Mr Marriott's 'practical' theory covers "all art." "All art is the record of human gestures in the presence of the subject or idea and as conditioned by the nature of the medium in which they are made." It seems to me this definition is at once (1) too complete and (2) incomplete. It is too complete, for it covers life as well as art. When a fly settles on your nose you wrinkle your nose and lift your hand to brush it off—both human gestures made in presence of the subject and conditioned by the medium, your own body. The record of these gestures would be, not art, but history. Similarly, with all the gestures, say, in Darwin's *Expression of the Emotions in Man and Animals.* Again, the definition is incomplete, because the word 'gesture' presents an incomplete image. Gestures of what? we say. Gesture is movement with meaning. It is the expression of something. It is the outward and visible sign of something inward and spiritual. And what can that inward and spiritual thing be but the very object of our search, the aesthetic fact?

Here we come to the kernel of the subject. It seems to me that you may go on considering gestures for ever and yet be as far as ever from possessing the aesthetic fact, the 'true inwardness' of art. The outward and visible sign will never conduct you to what is inward and spiritual. Broad washes will not explain to you the beauty of the water-colour, nor long stones, characteristically used, the beauty of the Parthenon. "Once accustom a man to the idea that art is primarily the characteristic use of tools and materials in view of the particular purpose, and you have given him a clue to the highest aesthetic appreciation." You will at best, I venture to think, have given him a clue to appreciation of the characteristic use of tools and materials. Hard by the Parthenon stands the little temple of Nikè; down below them in the town is the temple of Theseus. In each building the architect has used long stones characteristically; yet while that practical condition has never been lost sight of, three different beautiful forms have been produced, each of them an individual thing. How can you account for this save by reference to the artist's mind, his artistic impulse, vision, inspiration—call it what you will? Long stones used characteristically did not *make* these buildings what they severally were; the artist *used* them to embody his imaginative vision, the art in him. Take another illustration from a very different form of art—drama. It is one of the commonplaces of Shakespearean criticism that Shakespeare's plays were conditioned by the practical circumstances under which they were produced. They were produced on a 'platform' stage, and therefore show some peculiarities which would be out of place on our modern 'picture' stage—*e.g.* an excess of narrative and lyrical passages. The dramatic action of *Hamlet* is interrupted while the Queen lyrically describes Ophelia's death. Shakespeare makes her do this for 'practical reasons' (the conditions of the platform stage). But do these 'practical reasons' account for the aesthetic fact, the pure poetic beauty of the passage? Mr Marriott would have to say yes, for "practical and aesthetic reasons are at bottom the same thing." There you have a case where the artist has used his medium characteristically, as they used the long stones in the Parthenon; but the characteristic use of the medium would never have given you a clue to the beauty of the passage, which was contributed by Shakespeare, the poet. Further, Shakespeare had, like other dramatists, to consult the tastes and expectations of his audience. This was one of his 'practical' conditions. How did he meet it? Professor Raleigh puts the answer pithily. "The audience asked for bloodshed and he gave them *Hamlet*. They asked for foolery, and he gave them *King Lear*." Shakespeare, then, effectively complied with the practical conditions of his problem, as did the architect of the Parthenon with his long stones. But effective compliance with practical conditions will no more account for the beauty of the Parthenon than it will for the beauty of *Lear* and *Hamlet*.

Are we not forced then to the conclusion that this 'practical' investigation advocated by Mr Marriott, the consideration of 'gestures,' medium, material and tools, can never answer the fundamental question, what is Art? The enquiry from without having failed to bring us to the goal, may we not have better luck by trying the method of enquiry from within? After all, that is the normal method. The history of aesthetics through the ages runs mainly along that line. Aestheticians, from Plato onwards, have nearly all agreed in supposing that art is primarily a 'function,' as the mathematicians say, of the artist's mind rather than of his medium or of his tools. Instead, that is to say, of seeking to penetrate to the secret of art through broad washes and long stones, they began, so to speak, at the other end and considered art as a spiritual activity. One point of view, Mr Marriott's, says every landscape is the characteristic use of paint and brush in the presence of a corner of nature. For the other point of view you have Amiel's "every landscape is a state of the soul."

What is the dominant theory to-day among aestheticians who have pursued the traditional line, of considering art as a 'function' of the artist's mind? It is the expressionist theory of Benedetto Croce. "Art and Beauty are one and the same thing, and that thing is an experience of the human spirit. We might even say that it is the simplest of all such experiences, and the earliest. Whenever a human being concentrates his total feeling into an image which is at once its essence and its utterance, the spirit of art is awakened. Beauty, in other words, lives in the creative imagination, and there alone, and art is nothing more and nothing less than the experience which we call beauty. All else is irrelevant. True and false, real and unreal, good and bad, have no place in the aesthetic world. Here nothing counts but the perfection of the imaginative act in itself, and by its own standard. And this is in principle all that we need to know about the beautiful. It has no subdivisions within itself, and its manifestations array themselves in no kind of series. Aesthetic philosophy lies in explaining the scale of beauty among the experiences of the spirit, and in defending the simplicity of the principle against traditional but unjustified intrusions. This is the meaning of Croce's doctrine that beauty is expression. And that doctrine is sound[1]."

I have purposely quoted Dr Bosanquet's exposition of Croce because he is one of Croce's most formidable critics—and, as it happens, he is most formidably critical of Croce's handling of the very subject now under discussion, the relation between art and medium. For Croce this relation is purely external, a relation not within the aesthetic fact, but between the aesthetic fact, which is pure intuition-expression, and a particular moment in the historic situation of the artist. Croce holds—it is his cardinal position—that the aesthetic activity is purely internal, the expression within the mind of the artist's intuition. He may or may not, as he chooses, extrinsicate his expressions, communicate them to others in pictures, statues, and other so-called 'works of art.' But the real work of art is the internal expression, of which the picture or statue is only a practical record. Of course the artist does not work *in vacuo*. His aesthetic activity is exercised in a certain historic situation—at a given moment of time, in certain surroundings, with a certain mind and temperament. His intuitions will depend on the circumstances of his historic situation and, if he externalises them in 'works of art,' these circumstances will include his medium and tools and the joy (since every practical end successfully achieved involves joy) of "using" them, in Mr Marriott's phrase, "characteristically." But they remain always 'historic situation'; they are never 'art.' This abolishes the distinction between the arts according to the media or means of externalisation. "Since the individuality of the intuition involves the individuality of the expression, and a picture is distinct from another picture no less than from a poem, and pictures and poems are not of value for the sounds that strike the air and for the colours that are refracted by the light, but for what they have to say to the spirit in so far as they come home (*s' interiorizzano*) to the spirit, it is vain to turn to the abstract means of expression to construct the other series of kinds or classes; that is to say any theory whatever of the division of the arts is without foundation[1]."

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[1] Bernard Bosanquet, "Croce's Aesthetic," *Brit. Acad. Proc.* IX.

Of course the 'man in the street' will never swallow this. With him, perhaps, it would be idle to argue on a question of philosophy. But the man in the street has on this issue a potent philosophic ally, Dr Bosanquet, whose high authority as an aesthetician makes it impossible to ignore him. He is all for the old division of the arts. "After all it is necessary for philosophy to study the specialities and limits of the arts, for in considering them we are considering the qualities and aspects of the human spirit; nay more, of the spirit which is in the world. If you know or can feel how the beauty of bronze—that is, its expressive capacity—differs from the beauty of marble, you are on the way to understand the diverging beauties of the arts; and if you insist on neglecting these affinities of the spirit, your theory remains abstract, and has no illuminating power[2]. No illuminating power! For my part, I find a theory which (apart from its general applications) unifies all the so-called 'arts' and shows me their common basis one of the most "illuminating" in the whole history of aesthetics. Not that Croce denies—who could?—the *practical* value of the old classification of the arts. Indeed, he expressly insists on it[3]. What he denies is its *theoretic* value: its place in a philosophy of aesthetics. "To reject," says Dr Bosanquet, "the function of the body—our own and nature's—is not to honour but to bereave the spirit." But Croce does not reject it; he simply removes it out of a particular category.

Elsewhere[4], Dr Bosanquet, I submit, does injustice on this point to Croce by an imperfect quotation. "Croce says that the artist has every stroke of the brush in his mind as complete before he executes it as after. The suggestion is that using the brush adds nothing to his inward or mental work of art. I think that this is false idealism. The bodily thing adds immensely to the mere idea and fancy, in wealth of qualities and connections." Now what Croce says (*Estetica*, Ch. XIII, end) is: that the artist "never gives a stroke of the brush without having first seen it with the imagination; and if he has not yet seen it, he will give it, not to extrinsicate his expression (which in that moment does not exist), but as it were on trial, and to have a mere *point d'appui* for further meditation and internal concentration." The stroke of the brush, that is, gives him a new 'historic situation,' from which he will have a new intuition. The bodily thing, then, does not 'add' to the idea and fancy; it creates a new *milieu* for the idea and fancy to work in and so to produce new expressions. Croce is trying to isolate the aesthetic fact in the idea and fancy; Dr Bosanquet says no, you must include the bodily thing, the *milieu*, in it, too. According to Croce, "the old aesthetician Baumgarten advised poets, as a means of promoting inspiration, to ride, to drink wine moderately, and, if they were chaste, to look at pretty women." Schiller, the common anecdote goes, used to smell rotten apples for the same purpose. These are all 'historic situations.' Would Dr Bosanquet include them in the aesthetic fact, too?

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[1] *Breviario di Estetica*, 71.

[2] Bosanquet, *loc. cit.* 12.

[3] Croce, *op. cit.* 72, 73.

[4] Bosanquet, *Three Lectures on Aesthetic*, 73.

Finally, there can be no question of tabulating the consequences and advantages of the Crocean view of art, as Mr Marriott does with his view, because that would carry us far beyond the limits of the present discussion—the relation of art and medium. It will be seen, however, from what has been said, that Croce sweeps away not only the false distinctions that Mr Marriott abolishes but also those that he retains; not only the distinction between representative and non-representative art and the distinction between the various forms of the same art, but also the distinction between one art and another. Further, he not only, with Mr Marriott, brings all practitioners of the same art into the same category, but *all men*, in the act of expression, into the single category of the artist.

MIND AND MEDIUM IN ART[1]. (III.)

By HENRY J. WATT.

As in so many of the general problems of art it is important to coördinate discussion that springs chiefly from the arts of vision with their less substantial sister. In music, at first glance, we might be tempted to say there are no tools and materials. The composer records his inspirations upon paper with a view to legibility only. He elaborates in imagination the effect he desires, and restricts his record to the minimum required by the performer for adequate reproduction. The performer in turn must be able to imagine from this record, at least approximately, the effect intended, and he will then be able to produce that effect for others on his instrument, voice, violin, piano, etc. He may in some cases be able to enjoy it completely apart from any real instrumental performance. But this power clearly presupposes very perfect acquaintance with the sounds produced by a performer on the instrument for such a record. Hence clearly the musical instrument is a necessary medium in the progress from composer to enjoyer. So instruments are the materials of music. Perhaps the performer might be held to be the tool. But there is no value in carrying the analogy into such detail.

The instrument has long been recognised as an important condition of 'style'[2] in music. The human voice is a very special instrument with its own great virtues and limitations; it cannot be treated by a composer as if it had the freedom and range of a violin or organ, or as if it were devoid of that marvellous power of changing its timbre from instant to instant at the behest of the singer, a power lacking (? almost) entirely in the piano and only imperfectly attained in the organ at enormous expense. Orchestral works can be *studied* at the piano, but they must be heard or imagined in the orchestra. Berlioz—great genius of orchestration—relates with scorn how in his contest for the *Prix de Rome* he was required to produce a work for voice and orchestra which was judged by reproduction on the piano, while the winning work was properly heard only after judgment had been given ("*par des gens qui ne sont pas musiciens*")[1]. All this is probably commonplace amongst musicians.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] Cf. Sir Hubert Parry's *Style in Musical Art*. London, 1911.

Would it were so amongst the great public and the performers who meet their taste! Every other *virtuoso* regales us with Wagner's Pilgrims' Chorus on the piano, an instrument in which that work produces only a farcical effect. Just because it is designed for the orchestra, it is obviously difficult to play on the piano and so, in those for whom essential beauty is a secondary or remote consideration, it excites wonder and applause. The translation is as appropriate as would be the embodiment of Laokoon in glass or straw. To write piano pieces round the melodies of Schubert's loveliest songs, as Liszt did, shows an equal lack of taste. Anyone who really knows and appreciates the song could only be bored and disgusted with the piano's feeble echoes of the rapture of "Hark, Hark, the lark," or the tempestuous terrors of the Erlking.

And it is quite easy to account for the characteristic style of each instrument. The organ has always been and must ever remain a somewhat lethargic instrument incapable of producing intensive stresses. But it gives sustained tones of great variety. So the composer has perforce to build his works with tones of certain intensive, temporal, and colour values. The bricks and mortar of piano works are very sharp and short, but intensively malleable with a rather limited and mysterious range of colour. The violin has almost complete freedom in action, sustenance, stress, and colour. These things are familiar and need only be briefly recalled.

Let us now turn to Mr Marriott's thesis, which seems kin to the thoughts of Semper and others[2]. Are practical and technical reasons the same thing as aesthetic reasons? The answer cannot be a simple yes or no. We must distinguish two aesthetic processes—aesthesis, or act of enjoyment, and judgment thereon. Judgment we may at once put in its place. It is always cognitive; it always implies a conceptual apprehension of the objects, predicates and relations involved in it, whether these objects be things, sensations, or mere subsistents, and whatever the predicates and relations may be. The aesthesis is not so easily placed. It is of course always an act involving a 'relation' or unifying process of experience. It cannot be mere sensation for example; for then it would be indistinguishable from the sensory data of cognition or of instinctive action. Nor can it be merely subjective, as if the objective counterpart of it were irrelevant, i.e. the tones, colours, forms, of the beautiful object. Beauty cannot be skimmed off all its actual forms in a pure essence in which they play no part. Beauty like knowledge always attaches to some object.

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[1] "N'est-il pas évident que le piano, anéantissant tous les effets d'instrumentation, nivelle, par cela seul, tous les compositeurs?" *Mémoires*, vol. I. chap. 22.

[2] Cf. E. Meumann, *Aesthetik der Gegenwart*, Leipzig, 1912, 125 ff.

The analogy of knowledge might tempt us to suppose that there is a 'logic' of beauty, shall we say "intuition expressing itself," as there is a science of the pure concept and its relations[1]. It is quite legitimate to make abstraction of the forms and laws of any range of cognitive processes from their (integrative) basis, as logic abstracts from the basis of any meaning in my mind or yours at one and another moment. But that need not lead us to suppose that logic is completely independent of knowledge and psychology. We must land ourselves, as the greatest have done, in a philosophical *cul de sac* if we do not realise that all knowledge is one great system in which the highest grows continuously out of the lowest. The crown is not arbitrarily placed upon the head of sense or assumed by it. The kingship is inherent.

In beauty we shall hardly find a parallel to this. There must always be an act of grace or marriage between the objective and subjective in beauty, a marriage of true minds, born for one another and finding each other's nature predisposed for harmony. We have to discover the secret being of this harmony that is the relation between subjective and objective mind. Namely, what is that rhythm of interaction between tones in all their variety, groupings, and sequences, between colours, forms, and motions, between smells, tastes and other limited resources of the simpler senses, on the one hand, and the pulses, acceptances, anticipations, appetites of the subjective mind, on the other, that appears or is for us pleasure? How shall we describe the collisions, refusals, divagations, and distresses between these two streams of actions that is displeasure? A pure science of these things is hardly yet in existence. Until it has been fully and intimately grounded in the detail of knowledge, I do not see what value such formulas as the "expression of intuition" can have. These terms are so vague and general and their whole atmosphere is so inimical to the detail without which general propositions (except in logic) dare not be advanced, that they can only be set down as anti-science and obscurantism.

Having thus divided the aesthesis into three parts, we may notice that our knowledge of the subjective side is exceedingly vague and schematic. Our knowledge of the 'relation' that is pleasure-displeasure is equally feeble. The objective side alone is open to inspection with moderate ease. But even here there is great controversy. What especially is included in this objective field? A great wave of opinion has drenched us with the dogma that only sensory data and complexes are admissible. Away with all 'subject' and association! No perceptual, reminiscent, associative, literary, anecdotal, thematic adhesions are to be tolerated in any art (*scilicet* visual art). Colours, forms, times, spaces (indicated),—these and the like alone are fit elements of a work of art.

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[1] This is the thesis towards which Mr Walkley inclines, though I am not at all sure that he really means it to be taken in so extreme a form.

But there is a good deal of perversity in this dogma in spite of the earnestness and desperation with which it has been preached. For music has within the same period been striving to attain the very opposite goal, namely to force upon its medium perceptual adhesions that can hardly be induced even to cast a shadow in the desired direction. This is another thing that makes it so important always to compare the visual and auditory arts. They are so differently constituted that they are nearly always trying to do opposite things. Vision gets tired of being representative and tries to be as directly divine as music, which at the very moment aspires after more and more contact with the things of earth. Vision longs to be rhythmical while music wooes the rhythmic freedom of adagio[1] and might even yet become a new unmeasured music. Vision makes frantic efforts after a pure harmony of colour, when music tries to shake out of itself the last traces of modulated sweetness in order to be more characteristic of things that are not so clearly inherent in its nature. Of course it is well to create an art of characteristic noise, and it may even thrill our being; but it no more affects the course of music that has been building itself up in a steady progress through the centuries than the beauties of prose annul and negate the much older and still potent charms of poetry.

So we may pass over the attempt to create a dogma of limitation. The arts of vision in the greatest creators have known no limitation to the purely sensuous range of data. They have always freely admitted whatever furthered their virtues, felt as these are in the immediate poignancy of the aesthesis. In the Winged Victory, the Laokoon, the Creation of Adam, the Last Judgment—there is much more that is of the greatest aesthetic force than ever the mere eye can see.

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[1] "Hier steht nun das Adagio dem Allegro gegenüber, wie der gehaltene Ton der figurierten Bewegung. Dem Tempo adagio giebt der gehaltene Ton das Gesetz; hier zerfliesst der Rhythmus in das sich selbst angehörende, sich allein genügende reine Tonleben.... Was im Allegro der Wechsel der Figuration ausdrückte, sagt sich hier durch die unendliche Mannigfältigkeit des flektierten Tones." Wagner's *Gesammelte Schriften*, Leipzig, 1907, vi. 284.

But the difficulty is to show how these perceptual moments link up with the purely sensory data to form the objective stuff of art; we think of course that we understand all this perfectly, simply because we know which colours to put together, which tones to group and to serialise. As one of my critics tells me, and as many others think, "The rest may reason and welcome; 'tis we musicians know." But they do not know. Nor do the colourists know why one colour, one form, one space goes with another. Guesses can be made and some accord fairly with the little direct psychological knowledge there is. But we are far from the needful pure science of these things. So long as this is so, what right has anyone to say (in the face of the practice of the great masters) that perceptual processes do not belong properly to the objective stuff of art? Of course I should not dispute the fact that some subjects are tawdry and dangerous, and great regions of non-perceptual art in vision may still lie unexplored. For a medium sets limits and in doing so it opens up channels closed to other media. The question concerns the canons of art only.

The difficulties of the problem may be exposed in another way. Colours, forms, tones and their groupings, etc., may obviously be beautiful. But, tell us, can a subject possibly be beautiful? Frankly, is it conceivable that a thought, an idea, a perceptual substance-unity (the orange without the colour, shape, smell, weight, etc.—what is left over), or an association, can be beautiful, *schön* ('shining')? Never! Hence let subject be for ever banned from art (and dragged into music).

Let us answer this by a counter-question. Why are colours and forms beautiful? Because they 'shine' and grip the eye, sending a thrill into the soul through a pore of that sense, as it were? Beauty streaming to you straight from the canvas and labelled such from its very start? Surely not. Tones are not felt to be 'out there' at the piano, in the singer's throat: they are everywhere, nowhere; they do not 'shine'; they merely blend with one another in your being so that you think they and your soul are one (or some other such vagueness). In fact, again, we know next to nothing of the basis of these sensuous delights. And as we succeed in laying bare their nature, they seem strangely to evaporate into things that look like relations, clearness of sequence, repetitions, variations, and echoes, that all seem so like those things with which only the conceptual intellect can deal adequately. But this last conclusion would be very wrong indeed. They are clearnesses and distinctnesses and recurrences of sensory stuff that strike and thrill our subjective mind to its responses. They are not conceptual relations of the intellect. This is a mistake made by most prominent thinkers about art. It has led one of them recently to assert more or less definitely that a person who does not *know* all that is going on in a musical work, the tones, intervals, chords, keys, modulations, reminiscences, etc., can no more properly enjoy it than he could enjoy poetry in an unknown tongue[1]. That I am persuaded is quite wrong, untrue to the great and spontaneous joy that the greatest art gives even to the inexperienced provided they are 'pure in heart.' One could hardly dispute that the trained and swiftly judging mind must reap the greater and fuller joy from the art. But the germ of enjoyment is untutored. If it were not so, no intellect could bring it to birth.

If, then, we do not know in detail the roots of beauty in colours and forms, how may we deny that beauty can also spring from perceptual, conceptual, memorial and even emotional constructions? We simply cannot.

But it seems perfectly clear and certain that without a basis of sensory beauty there can be no work of art. Pure intuition or no, a work of art must be created, it must be embodied. And beauty must permeate its whole being, sensory, perceptual, imaginative, from whichever end of the series its soul may spring.

We may now return to the problem of discussion. That now takes form in the question—are there any limits in art to the range of perceptual and other adhesions to the sensory basis of the work? Do these adhesions include such as are concerned with the medium and its characteristic manipulation? Or does the range of the latter out-reach the former? For, of course, it is out of the question that actual matter and its manipulations could bodily enter the aesthetic object, which is always as apprehended and known, i.e. essentially mental or spiritual, as Mr Walkley properly recalls. The sole question is,—are these perceptual and cognitive attachments irrelevant to the scope of the aesthesis or are they not? And my answer is,—they are not and never can be irrelevant in good art, i.e. *quâ* in a good spectator. But on the other hand there is no means of fixing a norm for the extent of them that a good spectator shall have at his disposal. That is simply another form of the truth often so crudely distorted that powers and therefore tastes differ. The judgments by which they express themselves cannot be disputed any more than can any other facts; but their basis and general validity can be discussed.

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[1] C. Stumpf, "Die Attribute der Gesichtsempfindungen," *Abh. d. preuss. Ak. Wiss.* 1917, VIII. 56 f. "Wie dagegen ein wahrhaft künstlerischer, *verstehender* Genuss unserer harmonischen und polyphonen Musik ohne *jede* Zerlegung, jede Beachtung des selbständigen Ganges der Stimmen möglich sein soll, wäre schlechterdings nicht einzusehen. Dabei wollen wir gar nicht bestreiten dass auch ein Dahinströmen ganz unzerlegter Klangmassen durch das Auf- und Absteigen des Ganzen und durch den Wechsel der Klangfärbung und der Zeit- und Stärkeabstufungen gewisse 'musikalische' Stimmungen hervorrufen kann. Der Geniessende mag dabei der Musik gegenüberstehen wie etwa den Farben der Kalospinthechromokrene oder wie einer ihm zwar unverständlichen aber mit Effect gesprochenen Rede." My analysis of the primarily melodic functions of music shows that this account is far from the whole truth of the 'natural' apprehension of music. An exact parallel to this applied to the 'natural' enjoyment of a picture or of dancing would surely be ridiculous. Debussy on the other hand dreads in the auditor a knowledge of means. Does not this cast some doubt upon their dignity?

We are now merely repeating what was noted above that the spectator brings his mind and all its sensibilities into the aesthesis. Much of this mind, namely the perceptual, associative, conceptual 'forms' that apply themselves to the sensory stuff offered, become simply continuous therewith and form the objective mind of the aesthesis. The rest of the mind's dispositions are free to respond to this presentative complex and to play with it until its interest is exhausted. You cannot show marble, bronze or wood to a human eye without its perceiving the material and thinking about it in some way or another. Whatever thus accrues to the aesthetic moment must either favour or hinder the aesthesis. Being intimately bound up with its objective part, these adhesions cannot remain indifferent to it. They will either agree with the colours, forms, and manipulated textures of the bronze, or contrast with them. Some minds are pleased by contrast; it arouses their wonder and astonishment. Perhaps this motive makes an appeal in the choicest circles more frequently than is commonly supposed. Has not a notable composer of ours[1] declared that novelty is the essence of the beautiful? Does not the present age of strenuous inspiration try desperately to produce wondrous effects, using devices that many of the previous masters may well have rejected in principle? But of course this makes a much more artful wonder than the bottled schooner in full sail or than the veiled figure in an ivory piece. The main trend of the beautiful seems, however, to keep aloof from this fringe of the marvellous in mere technique. No doubt a great deal of pure joy can be got from a sense and knowledge of

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[1] Cyril Scott, *Philosophy of Modernism in relation to Music*, p. 3: "In order to create something beautiful, one must create something new." Mr Scott chooses a mid-way ("Romanticism—newness within limits") between classicism and futurism and in a certain sense his statement is a truism. But it is surely strained to imply, as his examples suggest, that the newness that is beauty should always reside in some general feature or style of work.

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the nice operations in a work of art. But surely the first ground of the aesthesis in sense must be the natural sensuous coherences themselves of colours, tones, forms, and motions. And from this ground progress towards ideas and emotions should in the main be direct, not swerving far aside into special perceptual tracks, unless it intends thereby to return to the final reaches of mind with greater force and appeal. Nothing can of course prevent the normal range of perceptual and other associates appearing, while greater experience in art will inevitably refine and extend this range in special directions. Thus a work must appeal differently to the tyro and to the expert. And there must be a world of artistic objects for all grades of experience. Are we not nowadays learning to 'appreciate' the art of the negro, the primitive, and the peasant? Thus art must always allow for and largely first build upon the normal contents of mind and memory.

And it is perhaps one of art's most telling devices to appeal to mind by some special deviation from the currents and expectations of the normal mind. In this sense it may be true that novelty is the essence of art. I would rather say that the purpose of the novelty is to direct the mind of the spectator into a special direction. But this device will hardly extend to the usage of the medium of art in ways that are unnatural.

In summary we may say that the objective aspect of art cannot be limited to strictly sensuous data. It must be extended to include any 'objective' that is adequately founded upon these data. Hence the largely conceptual arts of literature. The spectator's awareness of all that pertains to the material of art and its manipulations thus comes within its scope. But I fail to see how one could reasonably hold that of all the non-sensuous objectives of art only those appertaining to the material and its manipulations should be admitted. Zeal for an important and perhaps neglected part may blind one to the rest, just as one's contempt for a greatly exaggerated value (e.g. of subject) may lead one to depreciate it unreasonably. The field of art is the whole soul and its congruences,—and its conflicts, too, its dissonances as well as its consonances (provided we can use them 'paraphonically'). It is the extreme sweetness of many subjects that has made subject such taboo. Great skill is needed to make art of these pretty ('easy') beauties. The discords of life that also appeal so strongly to our emotions need similar skill of treatment. And just as music gains more perhaps from the progressive conquest of dissonances, so the greatest wealth of the arts of subject has probably always lain on the side of the dramatic and tragic and less than pretty. But in the sphere of the perceptual associates of the medium of art, there seems to be no analogue of this; we do not see great artists growing more artful in the false, perverted, and imitative use of materials and tools, nor does the art seem to flourish that scorns technique, perverts good form and flouts good taste for no artistic end but merely for the love of novelty.

Music is comparatively free from these remoter influences of tools and materials. In the visual arts perceptual associates can be involved that are not at all visible but merely known, as one knows that the arms of a marble cross bear great strain, which again one knows is a natural virtue of any sort of wood, even of a tree branch. In music probably every aspect of the instrument that is artistically relevant is actually heard, as the grain of wood and marble is seen. Do we ever genuinely find a work the lovelier, the better its style suits the mere instrument, as Chopin's music suits the piano? Is it not enough to say that his music suits the tones that are typically produced by piano action? An auditor does not seem greatly urged to go beyond the heard sound and its typical characters to the thing that produces them. He may have special interest in instruments and he may know them by their tone. The different trade types of piano differ greatly in their timbre, but even acoustic specialists could hardly say at all precisely why they differ and no aesthetic listener probably takes any thought of the point of hammer blow into his aesthetic moment. And yet this is clearly a matter of the characteristic use of an instrument. I think one may take it that the finest violin music may be completely enjoyed without any thought of the material, manufacture, technique, and environment of origin, of the performer's violin. So although there is a vast amount of the greatest interest to be known about instruments in relation to music, I doubt whether it has any necessary aesthetic value whatever.

But perhaps we do wrong to mention the instrument in this connexion. We might rather think of the performer's vital, beauty-expressing movements than of the mere mechanism that carries them to the sounding strings. Should we have thoughts of Pachmann's finger-action and pedalling in order fully to savour the beauty of his touch? Must we first know how to play the big drum and cymbals, the bassoon and viola, ere we can hope to exhaust the loveliness of a symphony, and must we think of the playing of every instrument while we listen? Surely not. Such considerations are probably of little influence in music; certainly far less so than in the visual arts. Tones in a sense are divorced from material and are not shaped even by the tools that physically determine them. They are entities of independent being, divine as folks say, again.

But in thus discarding the most of the perceptual and memorial associations of music and its instruments and performers from co-operation with the aesthesis, I would not be thought to set up any limits in this respect. Such limits cannot be set up in the visual arts where association is more direct and powerful. It is not necessary to have any fixed stock of technical thoughts, but everyone has many of these and they are naturally prominent and influential. In music such thoughts also exist, but they are not naturally responsive and influential during aesthesis.

Finally, I think a protest should be made against the attempt to turn art into a kind of utilitarian practice. Art may well have originated in the useful crafts and it has something in common with the gestures and expressions that are of social utility. But just as the study of mind from the biological side has in the primates had to recognise beyond hunger, love and play a delight in mere sensuous or intellectual effects, as when a monkey plucks at a vibrating pin or a child beats a drum indefinitely, so the science of art must rise above the biological cant that was natural in a recent generation and restore even to popular favour the idea that art is one of the three supreme ends of the spirit, a delight in congruous (true, good) being. After all biology now proclaims loudly that an organ is first formed and then, may be, finds a use (the world has not much time to waste on useless things). Well, then, art and knowledge and morality have first been formed and have been put out on loan. No amount of use or habit-craft and no amount of mere 'need' would ever have brought them into being. And so no matter out of what actions or organs they arise and no matter how they are used, they are values essentially independent of these attachments. Mr Marriott's maxim can only be true if he means by characteristic just *aesthetic*. If, however, he means to suggest that what is in some sense objectively characteristic is found by us to be also aesthetic, does he not go far to show how the values of art may be objectively or *really* grounded and permanent, as knowledge is?

It may be the recognition of this that has drawn Mr Walkley towards the nerveless abstractions that Croce puts in the place of the theory of art. Although knowledge and beauty are unique phases of the spirit, they must be used by the mind as a whole. The new essence they bring to birth is not degraded in the least by any interaction with the other level of mind. It must surely be evident that things cannot be taken out of the personality or imagination or intuition of an artist and embodied in sights, sounds, or motions, unless these vessels can *contain* them. Here we come upon the majestic truth that "I and my Father," the work of art and the artist's 'personality' to that extent are one and yet so completely two.

MIND AND MEDIUM IN ART[1]. (IV.)

BY EDWARD BULLOUGH.

I AGREE with Mr Walkley's criticism of Mr Marriott's proposal to base an aesthetic theory upon Medium and its technical manipulation. The attempt has been made more than once—the example of Semper has been mentioned by Mr Watt—and has always broken down in the face of objections such as are urged by Mr Walkley—in the last resort because Art is a manifestation of mind and cannot be satisfactorily accounted for by the nature and treatment of the material it employs.

The second part of Mr Walkley's paper rests upon the theory of Benedetto Croce. I am in doubt how far I agree or disagree with him. Croce's aesthetic theory does not stand by itself, as is far too commonly supposed. It is part of a general philosophical, and in addition monistic, system and is exposed to all the difficulties created by such a dependence. I feel doubt about the fourfold division of the activities of the spirit; about the precise limits of the theoretical and practical activities; about the ultimate implications of his general point of view; as regards the aesthetic sphere in particular, about the nature of the 'intuition' and the essential 'lyricism' of Art, and their consequences. Indeed, I am inclined to agree with the objections recently urged by G. A. Borgese in the Preface to the new edition of his *Storia della critica romantica in Italia*[2]. I readily admit that Croce's conception of intuition as the

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] "Compiutosi in De Sanctis lo svolgimento della critica cosidetta romantica, due vie restavano aperte; o attuare ulteriormente e più risolutamente quella restaurazione del senso classico dell' arte che in Italia paradossalmente si chiamò romanticismo, o, traverso quello pseudoromanticismo, pervenire ad un romanticismo autentico.

Il Croce s' avviò, senza notevoli perplessità, per questa seconda strada, e sempre più si raffermò nel considerare l' arte come instinto, come 'Genie,' come irreducibile individualità, come 'hohe Intuition' anteriore alla logica e alla volontà, come liricità pura, come interiezione, come grido. Il suo metodo critico si andò sempre più precisando in tre canoni: (1°) negare qualunque continuità di sviluppo nella storia dell' arte, tutta frantumandola nelle monadi senza finestre delle singole individualità poetiche; (2°) cercare in ogni poeta l' opera fondamentale e genuina, generalmente di prima e spontanea gioventù, rifiutando come sovrapposizioni logiche e volitive i suoi pretesi progressi di maturità; (3°) leggere ogni poeta ed ogni opera come un' antologia in cui alcuni frammenti lirici

kernel of the artistic attitude, his antagonism to the current treatment of the history of Art and to the sterile discussions of the differences and relative position of the arts, and his insistence on the unity and uniqueness of Art, etc. have rendered immense services to aesthetic studies; yet I cannot but feel a certain sympathy with the complaint of Professor Bosanquet (with whose aesthetic views I otherwise disagree) that Croce's theory yields little enlightenment on the particular subject now under discussion. Croce's theory is a philosophical, not a psychological, statement, and there is much that seems psychologically obscure in it. Moreover, the subject of the present debate is in itself, as a matter of psychological fact, so obscure that its elucidation would appear to me as little furthered by a philosophical treatment which deals with it from an ideal point of view, as by Mr Marriott's hypothetical illustrations which substitute assumptions for experience.

I. What do we actually know about the relation of Mind and Medium in Art?

By *Medium* I assume we mean the material or physical datum which the artist handles: paint and canvas; chalk and paper; marble or bronze; brick or stone or concrete; the tones of an instrument or the human voice; the human body, its gestures and attitudes, whether in actual space as in dancing or acting, or in imaginary space as in the cinematograph film.

By *Technique* I mean the manipulation of these media.

The *Mind* refers, I take it, to the mind of the artist as well as of the spectator or listener (recipient), but chiefly to the former.

As regards the term *Art*, I must beg leave to make one or two more extensive observations. In the first place we would, I believe, all agree that Art represents but a segment of the whole range of aesthetic activity. The aesthetic attitude embraces the appreciation of natural objects as well as of artefacts; it covers certain borderlands of Art such as ceremonial and ritual which have admittedly artistic affinities; it permeates ordinary human intercourse in what we vaguely call 'manners.' As a matter of personal conviction, I hold that there is nothing in the whole range of personal experience from sheer sense-experience to the most abstract thought which may not be the object of aesthetic contemplation. To speak of medium in this case becomes exceedingly difficult, because the medium is, I presume, one's very self—as, in fact, it is in certain

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siano artificialmente ricuciti per l' illusione extraestetica di raggiungere l' unità. Con particolare evidenza i suoi saggi recenti, specie quello su Goethe, mostrano la volontà di sottoporre quasi ogni dramma, quasi ogni poema all' analisi disgregatrice che la filologia romantica sperimentò sui poemi omerici." (pp. xix–xx.) G. A. Borgese, *Storia della critica romantica in Italia*, Treves, Milano. 1920.

types of acting: *la plastique de l'âme.* Of this vast realm of sometimes most tenuous and elusive experience, the title restricts us to the most condensed—if I may use a contradiction in terms—the most material, but also the most deliberate and most intentional form of the aesthetic state.

II. Further, I believe it to be important to point out the distinction between *Art in its static* and *Art in its dynamic aspect*. The distinction is important because, though it is constantly overlooked, it affects the relation between mind and medium in several particulars, and incidentally also the conception of a history of Art, the interplay of social and individual factors and the continuity of artistic and aesthetic tradition.

By Art in its static aspect I mean what I cannot do better than describe as "the objective world of Art," i.e. the accumulations of art-treasures in our museums and collections, the art-*objects* as material things, independently of all considerations of their subjective origins, the intentions and ideals of their creators; independently also of our aesthetic attitude to them, regarded merely as objects, as remains of antiquity or illustrations of the past.

Art in its dynamic aspect is Art regarded from the point of view of the creative artist or actively appreciating recipient: as an aesthetic object. There is of course a constant interchange between these two conceptions of Art. An art-object which to-day is dynamic for me, may to-morrow have become static. A picture may one day belong in my eyes to the objective world of Art and the next day may assume a dynamic aspect. To say that as a static object it had no aesthetic existence for me, is of course true—that is precisely the distinction—; yet I knew all along that it was an art-object and was regarded as such by others for whom it had become dynamic. And its claim to become dynamic for me also was there with it and exercised a certain moulding influence upon my aesthetic attitude. This silent, collective and largely anonymous pressure of the "objective world of Art" is probably the most potent agency in the art-education of the individual. It represents in its cumulative force, created by the forgotten but combined verdict of past generations—to whom it also owes its preservation—a tremendous standardising power upon the schooling of appreciation and produces adaptations, expectancies, apperceptions and emotional reactions which in their varying degree of rigidity and durability result in more or less fixed habits of taste. This applies to generations as well as individuals. The "objective world of Art" and dynamic Art are naturally never coextensive. For the majority of persons the vast number of art-objects never become dynamic; but the appeal to become so remains and is satisfied in proportion to the powers of imagination, the emotional elasticity and the extension of the organised interests of the individual appreciator.

The same standardising pressure is exercised upon the artist. The beginnings of every artist are necessarily imitative. What should he imitate but what was there before him? This necessity is reinforced by the capacity of absorption and of admiration which distinguishes the artist as a psychological type. It is further forced upon him by the perpetual technical struggle in which every actively creative artist lives. Defeat in this struggle means incapacity or mannerism. Indeed, it is precisely on the technical side that static Art exercises its strongest influence upon the artist, for it represents the continuity of technical development—distinct from, though not unrelated to, the aesthetic discontinuity of Art history—and technique is the one thing in Art that can be taught—up to a certain point. Incidentally static Art acts as a kind of background for the ricochet of general public sentiment and its attitude towards Art upon the artist, and as a reaction both to this sentiment and to the technical teaching beyond the point where in the artist's development technique ceases to be impersonal, common knowledge and becomes an intimately personal process interwoven with his artistic imagination, static Art becomes the spring-board of reaction, revolt and revolution. For all great artists have been revolutionaries, both in their technique and in their creative work as a whole.

In this manner, the objective world of Art, by reason of its persistence has in a sense a survival value, and as the expression of certain forms of historical continuity, represents for the artist and the appreciator a kind of collective and socially-conditioned standard and—up to the above-mentioned point—a similarly socially-conditioned, impersonal technical tradition.

As constituent elements of the objective world of Art we encounter the various 'arts' and crafts, as historically they have arisen, developed and differentiated themselves in their materials, techniques, styles and ideals, and have created different professional habits of thoughts and tastes and distinct expectations and anticipated standards in their respective devotees. Art as the formulation of a unique and distinct psychological attitude is unquestionably ONE, and these different art-forms, as I should prefer to call them, appear largely as the outcome of accidents of history, climate, habitat, economic conditions, inventions, cultures, and in the last resort, of our physiological constitution. But they are for all that aesthetically not irrelevant, however little result the past discussions on their aesthetic differences and relations have yielded. The error lay in regarding them as fixed, predetermined and logically deducible species, each with its own 'laws' and 'principles.' The art-forms which we know and distinguish conventionally, are not, of course, the only 'arts.' In other countries, at other times, activities which we do not regard as a form of art were reckoned among the arts and *vice versâ*. It is the subjective attitude which recognises certain activities as art-forms, and the equally subjective repercussion exercised by them, once (and while) they are so recognised, which make the 'arts' aesthetically relevant. Even at the present time we are witnessing in the development of the cinema-film, however much prostituted for the moment, the rise of an entirely new 'art,' and there is no limit but that of our physiological constitution to aesthetic imagination and its interaction with non-aesthetic inventions for the production of material art-forms. Nor is there ultimately any limit at all to the range of subject-matter of the aesthetic psychosis itself. The 'arts' are merely the forms of 'exteriorisation' which in the course of history have been relatively distinct and continuous strands in the art-tradition of mankind.

The influence of the media in producing changes in the technical procedures and thereby in the historical styles of the art-forms is so well known as to require but mere mention. The effect of small units of building materials upon the development of architectural forms and eventually upon the growth of the Gothic style; of Roman cement upon Roman vault construction; of reinforced concrete upon the structural problems and architectural forms of the present day are notorious instances in Architecture. Equally well known is the influence of tempera upon certain characteristics of Italian Renaissance painting and the changes introduced into the art by the use of oil-pigments. Comparable to the ease and freedom of oil painting have been the effects of the development of the piano on music, and the similarities of the consequences, both good and bad, have frequently been noticed. The differences between the use of marble and bronze in Sculpture, whether in the round or in relief, are to be found in every text-book on ancient Sculpture. The peculiarities of French verse in contrast to English, German, Italian and Russian are due for the largest part to the absence of a marked stress accent in the French language, and the use of intricate classical metres in English poetry is practically impossible because the language is so largely monosyllabic. Changes in the shape of the stage and in its relation to the audience, in decoration, *mise-en-scène* and personnel reacted on the nature of 'theatrical illusion,' types of play and the whole style of acting. The transition from the Commedia dell' arte to the regular drama had its repercussion on the psychology of the actor.

It would be easy to multiply *ad infinitum* such influences of media upon the art-forms. The point I wish to make here is that in changing the forms and traditional styles of the arts the effects of the different media are crystallised and incorporated by means of their techniques in the objective arts and, as stated before in more general terms, come inevitably to form the basis for the technical rudiments of the budding artist and set up new standards of technical handling in the mind of the recipient. One cannot but be impressed—whether favourably or not—with the profound imprint, for instance, of the 'classical orders' upon the mind of the architectural student, ever since Vitruvius has become the bible of the 'classical' architect. The extent and depth to which the student's vision is moulded by the shape, dimensions and proportions and formal functions of these orders is enormous with the cumulative force of century-old training on these lines. This is perhaps an extreme instance of the pressure of historical style, but similar results, if in smaller degree, will be found in other art-forms.

Mr Watt points out that the musical instrument and its technique is an important condition of 'style' in music. Indeed, 'style' has often been defined as the consistent treatment of a particular medium. This is true, if by style is meant what I have here called 'historical style.' But style in the purely artistic sense is not the consistent treatment of an impersonal, physical medium any more than technique is Art, but a consistency in the artist's personal vision, "consistency of distance" as I have called it elsewhere[1]. The manipulation of the material medium enters into this vision, no doubt; but it is not, either directly or by itself, the basis of 'style.'

III. What is the process by which the medium enters into this vision of the artist? This question leads to the consideration of Art in its dynamic aspect, i.e. to artistic creation and aesthetic appreciation.

What is the dynamic relation of the artist to his medium?

By way of apology for much that I have said above, I venture to submit that nearly all aesthetic theory of the past and of the present seems to me exposed to the serious criticism that it deals with the artist, so to say, *in vacuo*, psychologically and philosophically, without any reference whatever to the social unit, social relations and interactions in which he finds himself. This treatment reached its height with the Romantic Movement in Europe, and, as if to redress the balance against this romantic survival in Aesthetics, the type of theory called 'sociological Aesthetics' came into being, with its equally one-sided insistence upon Art as a social manifestation, as an 'expression of society.' This view, usually going back to Taine in some form or other, was supported by the development of scientific thought in the nineteenth century and reinforced by the progress of economic and anthropological studies. The truth seems to lie, as usual, somewhere midway. The artist is not a solitary being, living metaphorically upon a desert island, but is born into a community whose influences he undergoes in common with his fellows. In a sense he does so even more than the others, for a great susceptibility to ideas and the ambient atmosphere and a great power of absorbing them, exceptional both in range and rapidity, are part of his artistic psychological equipment. It is this impressionability which makes him so much more often than his duller brethren the unhappy victim of the herd and, by a natural reaction, into a really solitary person. Yet while *humanly* his contacts with his environment are at least equal to those of his fellows and are usually much more numerous and much less superficial, he is, *quâ artist*, undoubtedly infinitely less subject to social pressure than any other member of society, stands infinitely less in need of social co-operation for the exercise of his calling and is almost independent[1] of society in his artistic creation, development and ideals. As artist, he is a kind of imaginative solipsist. The relation of the artist to the Group appears to me consequently as very peculiar and very complex. It has never to my knowledge been satisfactorily dealt with either psychologically or sociologically. Yet it is of fundamental importance to Aesthetics both from the point of view of the artist's psychology and from that of Genetic Aesthetics which has to take count of the historical fluctuations of this relation for an understanding of the history of Art and the development of the aesthetic consciousness formulated in it. Regarding the special problem now before us, I must confine myself to saying that I believe the so-called 'art-conventions' of different ages which play so important a part in the treatment of media, to be a direct product of this relation.

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[1] This *Journal*, v.

(*a*) In its mere material aspect the medium plays a part in this relation, besides the technical tradition which represents one of the chief social influences upon the mind of the artist. The sheer materialness of a medium such, for example, as stone, its durability and conspicuousness, place upon the architect an artistic responsibility towards his contemporaries and posterity which is not borne by other artists in the same way or to the same extent. Building St Peter's or Westminster Cathedral, building for eternity so to speak, is psychologically a very different proposition to making an etching, composing a song or writing a poem. The permanence or evanescence of a medium invests a work with an atmosphere which reacts upon the psychology both of its creation and its appreciation, and while its *social* importance seems to increase in proportion to the crudeness of the material and the physical labour of its handling, the *individual* preciousness appears to rise to the evanescent tenderness of the human voice or the poignancy of a dramatic scene.—Again the power of experimenting, for instance, evidently decreases with the durability of the material with all the consequences for the psychology of the artist and the technique of the art-form, as in the well-known case of fresco-painting or lithography.—Even the personal relation of the artist to his work is bound to be involved. The poet may destroy his poem, the musician his song, the painter his canvas; they may be forgotten by the public; but building or statue remain to be seen, if not by the artist, yet by the public and become the point at which public feeling impinges upon the self-regarding sentiments of the artist. In a multitude of ways Architecture, Sculpture, Painting, Music, Poetry, Acting and Dancing appear as a series of art-forms in which the individual factor assumes ascendingly greater sway over the social determinations, in the freedom from technical tradition, in artistic responsibility, in public resonance; and the medium as sheer physical material seems to play a not inconsiderable part in the complex situation thus created.

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[1] Apart, of course, from economic considerations.

(*b*) I venture to touch in passing upon a point which affects the relation of mind to medium in its more external aspect, viz., the question whether the artist is distinguished by a special sensory sensitiveness towards the medium, superior to that of normal persons. Do painters possess a greater acuity of vision, sculptors a finer sense of touch, etc.? It would appear doubtful, if we allow for the specialised refinement of discrimination due to interest and practice[1]. The problem is obviously difficult to test, but without actual experiment we shall not be much wiser. It would certainly be well worth while to attempt special research on this point as well as on the superiority which training and practice unquestionably confer upon the artist.

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[1] Heine und Lenz, *Über Farbensehen, besonders der Kunstmaler*, Jena, 1907.

(*c*) On the psychical attitude of the artist to his medium, I had occasion in the course of some experiments on the aesthetic appreciation of colours[1] to make the interesting observation that painters and others professionally interested in colour assumed two entirely different relations to it, according as to whether they viewed it, so to speak, aesthetically or pictorially. As artists they displayed the same attitude which they would assume towards a work of Art; as painters they treated it with a kind of objective detachment as a mere, almost indifferent, instrument. The same dualism of attitude will, I think, be found in all arts. The architect or sculptor will feel a thrill of purely aesthetic enjoyment at the beauty of a finely grained stone and will lovingly caress the surface of a beautiful marble; the literary artist will gloat over the sonorousness and rhythm of a word; but each will, when the occasion arises, treat the same object of his delight as part of a completed whole with apparent indifference and will ruthlessly discard it from an unwanted place. And yet something of the intrinsic aesthetic value of the medium will persist as an almost imperceptible diapason in the full harmony of the work. The indifference of his professional attitude is not the indifference of the heedless layman, but the higher detachment which removes the medium from the grossly practical function of a 'materialisation' of his vision into an integral part of it.

(*d*) The connecting link between the medium and the vision of the artist is *Technique*. The relation described by that term is the adaptation of the medium to the vision, and, *vice versâ*, the adjustment of the vision to the possibilities and exigencies of the medium.

A. (1) Under the first section of this definition we find that Technique consists in the first place in a *knowledge of the medium and of its behaviour*. What is the limit of stress of an arch constructed of a particular material? To what extent will a marble or bronze permit the free detachment of a limb from the body of a statue? What is the range of the human voice? How far will a particular pigment darken in course of time? At the present moment reinforced concrete construction has introduced problems of its own under this heading which are far from solved. Much the greatest part of 'art-education' is concerned with the transmission of the wholly impersonal knowledge of this kind.

(2) As a special part of this knowledge appears the familiarity and practice with the instruments and tools for dealing with the material—a special part, because the invention of new tools has notoriously played

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[1] "The 'perceptive Problem' in the aesthetic appreciation of simple colour-combinations." This *Journal*, III, 444 note.

so important a rôle in the history of Art. It was for that reason that the mythical Daedalids were equally famous as mechanical inventors and as the first artists who "made men to walk," and we need but scan the technical sections of Vasari to realise the profound preoccupation of the Renaissance artists over the mechanical problems presented by the media of their day. A History of Art dealing pre-eminently with the modifications introduced by the invention of new tools and processes—in its repercussion upon the mind of the artists—would fill a gaping lacuna in our present knowledge and understanding of the development of Art, especially in its earlier phases.

(3) Knowledge of the nature and behaviour of the medium and of the functions of the tools for its manipulation will remain a mere impersonal 'knowledge by description' unless and until it is converted into active and personal familiarity by practice. The object of all practice is the acquisition of *manual dexterity*, if I may use 'manual' generically to indicate the muscular and motor-training whether actually of the hand, or of the control of voice, gesture, facial expression or even the remoter accuracy and spontaneity of auditive imagery, whether musical or linguistic. The ideal is "la man che ubbedisce all' intelletto" of Michelangelo, the perfect and minutely correct adjustment of muscular action to impulses of the will and even its unconscious control. This manual dexterity is the first prerequisite of any artist and is, in distinction to mere knowledge, transmissible only in the smallest part. The degree of perfection in the control of muscular movements that can be achieved is well known from the numberless stories told of most of the great artists of the past. Manual dexterity of this kind is one of the main differences distinguishing the artist from the merely appreciative layman. Though apparently nothing more (though often wellnigh incredible at that) than quasi-mechanical tricks—and a good deal of it is constantly mistaken for Art—it is the foundation, practically and psychologically, upon which great Art must necessarily build; practically, because it is the condition of the power over the material; psychologically, because it constitutes a personal knowledge of the limits and the possibilities of the medium which through the formation of technical habits comes to modify the artist's vision itself.

(4) Manual dexterity would be inconceivable without what I should like to call 'technical memory.' What I mean by technical memory is the accurate persistence in memory of the—presumably muscular—adjustments which enable for instance the actor to reproduce, perhaps after years, the same voice-inflection in a particular sentence, or enabled

Giotto on a famous occasion to draw a perfect circle free-hand. I have heard it said that in the last works of Beethoven the use of the human voice occasionally shows signs of a slight hesitation, due to the years of deafness with which he was afflicted. This would be an instance of the weakening of what I mean by technical memory. The existence of a technical memory, apart from its obvious necessity for the development of manual dexterity, is an essential condition to the exercise of technical imagination (see below).

B. If we consider the relation described by 'Technique' from the other, the mind-end, so to speak, we find, I believe, that it consists principally of the *knowledge of the effects* which can be achieved by the handling of a particular medium, a knowledge acquired, perfected and refined, of course, by the combined familiarity with the medium and the practice in its manipulation.

We encounter at this point a problem which I can mention only in passing. It appears in its acutest form in such art-forms as Architecture and Sculpture, though it seems to arise equally in the other arts. What is the difference between Building and Architecture or between a cast taken from a living body and a piece of Sculpture? The matter has been dealt with in the well-known book of A. Hildebrand, *Das Problem der Form in der bildenden Kunst*[1]. The point here is that in Architecture, for instance, the structure, the material form with all its technical problems, is, as it were, merely the substratum of the *spatial* forms which Architecture endeavours to produce and render effective. These two forms, the structural and the spatial forms, are by no means identical. The structural form may at times coincide with the spatial form, but very often the spatial form is quite different from, though produced by, the structural form. A column, structurally, carries a load and is the transmission of a *downward* stress; spatially, it is a *rising* form. Well-known instances are the usual geometrical-optical illusions, and, while Building is the science of structure, Architecture is the art of three-dimensional optical illusions. The architect aims deliberately at the production (or correction of undesirable) optical spatial illusions by means of the structural forms. Hence the fundamental importance of knowing what effects will or will not be produced by particular material forms in particular positions, perspectives, foreshortenings, lights, colours, etc. The same applies, *mutatis mutandis*, to Sculpture and all the other arts.

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[1] Jena, 1905 (5th ed.). There exists an American translation of the book, but the translation introduces an eye-movement theory foreign to the intentions of the original.

This is the point at which the vision of the artist is profoundly affected by the medium, for unless *the vision is conceived in terms of the medium*, it would be almost an abuse of language to call it vision at all. This influence penetrates the vision in its minutest details, in proportion to the perfection of the adaptation between the mind and the medium, i.e. of the technique of the artist. It is a crude but well-known illustration that a statue cannot be conceived in marble and executed in bronze. As has been pointed out by Max Klinger[1], it makes all the difference whether a picture is designed as an etching or as a mezzotint, i.e. whether you work from light to dark or from dark to light. The sculptural vision depends on whether it is to be carried out by modelling or by chiselling—"per via di porre" or "per forza di levare," as Michelangelo called it. You cannot write a play in prose and later turn it into verse without disturbing the whole relief of it, the interdependence of its parts, the colour of its sentiment and in the last resort the fundamental conception from which it sprang.

A failure to achieve this adaptation in the first instance is responsible for the constant corrections and alterations of works in the course of their technical execution, a source of the "développement par transformation" and "développement par déviation" of Paulhan[2]. Similarly the reproduction of works in media other than that for which they were designed is bound to be unsatisfactory and can at best have a merely illustrative value.

Lastly the sketches, notes, fragments and uncompleted works of great artists is the most valuable material at our disposal for the study of this interpenetration of medium and mind and are our best evidence for the rôle played by the medium in the formation of the artist's vision.

Having secured the fact of this interpenetration, we have to raise the question of its mechanism. By far the most illuminating conception here is that of the "images d'interprétation ou de traduction," suggested by L. Arréat[3]. He writes: "Un peintre figuriste, qui ne *sait* pas l'animal, a des chevaux à placer dans une composition. Il s'applique alors à l'étude du cheval; il prend des croquis, et se met dans la tête des 'images.' Un cavalier expert pourra ensuite critiquer son tableau en connaisseur. Il garde donc en mémoire, lui aussi, des images précises, auxquelles il a comparé celles du peintre. Il ne serait pas capable cependant de dessiner un cheval ni de le peindre. A quoi tient précisément cette différence? Il ne suffit pas de dire que c'est faute d'exercice, car la faculté même d'apprendre marque un véritable privilège. C'est d'abord faute d'*images d'interprétation* ou *de traduction*: j'entends par là des schémas visuo-moteurs laissés par l'étude dans le cerveau du peintre, et grâce auxquels sa représentation mentale peut prendre figure aussitôt sur le papier ou la toile—des symboles actifs, en quelque sorte, qui sont comme les idées générales 'pittoresques.'"

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[1] Max Klinger, *Malerei und Zeichnung*, Leipzig, 1907 (5th ed.), p. 34 ff.

[2] F. Paulhan, *Psychologie de l'invention*, Alcan., 1901.

[3] L. Arréat, *Mémoire et Imagination*, Alcan., 1904, pp. 28–29.

The interest and value of this conception appear to me to lie in the following points:

(1) It seems to me to provide a simple and lucid explanation of the process whereby practice and study succeed in developing, in the artist as distinct from the layman, both the wealth of memory-images and the facility for executing them.

(2) It gives a rational account of the fusion of the vision and technique and of the effect of technique upon—in the case of the painter—the visual imagery. For these schematic images or, as it were paradigms, forming the stock of the artist's imagery, have been developed in the course of practice and study by the accumulation of images *framed in terms of his medium.*

(3) It emphasises one of the main functions of art-education, viz. the formation and refinement of such 'images de traduction[1].'

(4) It offers an interesting explanation of what the French call 'la ligne de l'artiste,' the 'manner,' the almost impalpable peculiarities which distinguish the work of one artist from that of another. They would seem to be reducible to differences in their 'images de traduction[2],' their individual 'pictorial general ideas' under which each subsumes his isolated experiences and impressions. As M. Arréat remarks, a failure to develop, multiply and enrich his 'images de traduction' by constant exercise of observation (and especially imagination) leads to a sterile repetition of these images without individual elaboration: the 'manner' turns to 'mannerism.'

(5) The 'images de traduction' of the painter find their obvious analogies in the other arts, and the same psychological mechanism applies equally to them. Certain attitudes, certain turns of phrase, the use of epithets, the technique of character-portraiture, peculiar musical cadences and harmonies, peculiarities of gesture and voice-

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[1] Lecoq de Boisbaudran, *Training of Memory in Art*, Macmillan, 1911.

[2] Ugo Ojetti, the distinguished critic, suggests that the colour-schemes also form part of these 'lignes de l'artiste' (see Corriere della Sera, on the occasion of the 400th anniversary of Raffael's death).

inflection for the interpretation of particular emotional states, certain dramatic types—especially the old types of the Commedia dell' Arte—are instances of such 'images de traduction' in Sculpture, Poetry, Music and Drama.

So far the relation of mind and medium in regard to the artist up to that point at which he begins to develop his own individual art, his own imaginative powers, to become a creative artist and to leave the stage of a mere learner and imitator. Up to this point his accomplishments are not yet truly his own. The greater part are both socially determined and socially transmissible, for even his first 'images de traduction' will be derived from 'objective Art' and will be taken bodily from it, just as our—the laymen's—'pictorial general ideas' are far more extensively inspired by pictures than by reality. The foundations of his personal and individual powers are laid by the development of his manual dexterity and by the first imaginative elaboration of the original stock of his imagery. On the basis of these accomplishments, presenting the first fusion of the vision and the technique demanded by his medium, his creative processes are built up. The imaginative complication and enrichment of his conceptions is accompanied by an imaginative refinement and development of their technical formulation, and the farther his artistic imagination breaks away from the traditional stock of his art and becomes more and more his inner personal creation, the deeper and the more sweeping the changes which the simultaneously growing power of his technical imagination introduces into his handling of the medium. Thus new technical processes, new tools and methods, new ways of achieving effects and new solutions of material difficulties are discovered and minister in their turn to the wealth of his artistic imagination.

At this point we run up against perhaps the most formidable and fundamental problem of Aesthetics, that of the artistic imagination. In spite of much writing on the subject, we can hardly be said to have advanced beyond the fringe of the unknown. The chief obstacles to progress seem to me to have been a long standing confusion of imagery with imagination and the attempt to deal with imagination on far too narrowly intellectualistic a conception of novelty and originality as a characteristic feature of imagination. Imagination is clearly different from and in contrast to normal consciousness, but at the same time overlapping it to a large extent. The imaginative experience such as the artist in particular lives in, is more nearly the imaginative counterpart of actual experience than imaginings wholly outside the range of normal experience—which is true rather of people generally called 'unimaginative.' The novelty and originality of the artistic imagination lies far less in its excentricity to normal life than in its being the reflexion of an intensely and intimately individual experience, transferred to the sphere of imagination, thereby removed from its personal reference and rendered accessible to and effective for the sympathy, understanding and appreciation of others. This curiously dualistic, yet unified psychosis I have attempted to render intelligible as 'distancing[1].' And the medium, its treatment, its very limitations and the fusion with the artist's vision in Technique is one of the chief factors of artistic creation forcing the distancing process upon the artist while at the same time it facilitates the maintenance of distance.

IV. Concerning the relation of mind and medium in *aesthetic appreciation* little need be said. If the principle be accepted that, ideally, appreciation is a re-creation of the work in the mind of the recipient, it follows that the relation in his mind should not differ widely from that in the mind of the artist. The qualification implied in the term '*re*-creation' must not be lost sight of. For I confess that I disagree with the pleasant theory that "all men are artists." If we agree upon calling 'artists' those capable of creating Art, the best that we can say for the non-artist is that he is capable of following in appreciation the creative process of the artist, and his capacity even in this respect is notoriously limited.

Again, if, as I believe, the attitude to medium and its technique forms an integral part in the creation of a work, it may be contended that perfect appreciation should also be conscious and appreciative of this function. I cannot therefore quite follow Mr Watt in his statement about "the great and spontaneous joy that the greatest art gives even to the inexperienced provided they are 'pure in heart.'" Every appreciation of whatever kind requires a certain experience and education in the matter to be appreciated. I do not, of course, mean a reasoned judgment about it. But I am a little suspicious about the purity in heart. It savours to me of Ruskin and Tolstoy and suggests an uncritical admission into the effect of a work, as aesthetic enjoyment, of all kinds of pleasant impressions which may have nothing whatever to do with it.

I readily admit that you may aesthetically appreciate a work without knowing anything about its technical aspects. But I cannot persuade myself that an appreciation of this side would not materially enhance

[1] Edward Bullough, "Psychical Distance as a Factor in Art and an aesthetic Principle." This *Journal*, v, 87 ff.

the appreciation of the whole. To revert to the example of Architecture: an appreciation of the spatial forms is not necessarily dependent upon an appreciation of its structural excellence, as has been and is still contended by Viollet-le-Duc, Ruskin and their 'veristic,' moralistic and intellectualistic followers[1]. Structural excellence as such has nothing to do with aesthetic effect. What matters is the *relation* of structure to the space-forms, their subservience to spatial effects and the use made of them to achieve these effects; and I am convinced that an understanding of this relation will considerably increase and deepen the aesthetic appreciation of the whole, not least because it surmounts the dualism of these two sets of factors. But I equally admit the danger of technical knowledge for the layman and this is, I believe, what Mr Watt alludes to when he demands "purity in heart" of him. The medium and its handling have tremendous attractions for the layman, as all technical procedures have. The desire to *know* "how it is done" and all the unholy curiosity stirred by the relative ease of satisfying this desire by a purely intellectual knowledge about it are destructive of all aesthetic appreciation, unless they can be kept subordinate to the attitude of contemplation. The layman has generally neither the respect which the artist naturally feels towards his art, nor his consciousness of the true function of technique, nor his imaginative and emotional control. And it is, after all, so much easier to understand than to appreciate that it is little wonder if in the layman's mind technical knowledge takes precedence over aesthetic appreciation and thereby breaks up the unity of the aesthetic state. But this failure is no argument against the assertion that real appreciation of technique should enter into the total effect of the work with all the enhancement it carries with it, just as it entered into the creative act of the artist.

V. In conclusion I offer a few detached observations on the relation of mind and medium from the point of view of Genetic Aesthetics.

Genetic Aesthetics is the necessary complement to any psychological formulation of aesthetic facts. Psychological Aesthetics can give us an account only of what *we*, at the present time and with our largely local experience, mean by 'Art,' aesthetic appreciation and artistic creation, since only our own experiences are directly and to some extent introspectively accessible to us. What the Renaissance, the Middle Ages, the Greeks, Egyptians, Chinese, or the primitive peoples meant when they spoke (many never spoke) of Art is ascertainable only indirectly, on their own critical and theoretic statements[1]. Their works are no unambiguous testimony, doubly obscured as their evidence is by the confusing interpretations on our own analogies through which we have been accustomed to see them. That 'Art' has not, in its history, retained the same significance is beyond doubt. How far was the art-experience of the Greeks, Egyptians or Chinese the same as ours? How has art-experience in general come to be what it is? That is the problem of Genetic Aesthetics. No principle of Aesthetics can, I believe, be regarded as valid, which is not itself capable of evolution and thereby avoids the fallacy of applying to past periods of Art and human society terms which can have meaning only in reference to our present conceptions.

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[1] This point has been admirably dealt with under the name of "intellectual and moral fallacies" by Mr Geoffrey Scott in his *Architecture of Humanism*.

A. As regards Art in the objective sense, it seems to me that the importance and prominence of the material and of the technical tradition have noticeably decreased.

The material itself clearly plays a very considerable part in the estimation placed upon art-objects (if they can always be so called) in primitive 'Art' and much later, even latest, periods of Art-history. The value attaches apparently either to the intrinsic preciousness of the material—something of the kind survives in some of the crafts such as gem-cutting and work in precious metals—or to its magical efficiency, or to the mere difficulty of working it. None of these criteria are in our sense aesthetic, but entered, it seems, largely into the appreciation of former 'Art.' The extreme hardness of Egyptian porphyry probably added much to the value of works executed in it and the reference to it by Vasari[2] suggests that the difficulties of working it even in his day contributed to the estimation of this medium. White jade was reserved in ancient China for the exclusive use of the emperor, on account partly of its rarity, partly of its magical associations, and came to be regarded as the most precious kind of this highly prized material. The chryselephantine statuary of the Greeks must have enjoyed the reputation it had for similar reasons, for we cannot, I think, avoid the suspicion that artistically it must have been an error of judgment.

Technical tradition was notoriously rigid at certain periods to an extent which we would consider inimical to the production of anything that we should call Art. The reasons are very multifarious and cannot be dealt with here. Stereotyped patterns—stereotyped even so far as to be transferred to media intrinsically unsuitable to them—stereotyped arrangements of colours, of formulas, phrases, cadences, etc., are too well known to be more than mentioned. Ecclesiastical and any kind of hieratic influence has always been responsible for such an unbending maintenance of technical tradition, originally no doubt from magical motives. Wherever present, such motives inspired the worker with a quasi-religious awe and helped to prevent the mergence of the medium in his own conceptions, provided he were allowed to entertain such at all. Few artistic phenomena are in this respect more illuminating than Russian Ikon-painting with its rigid adherence to traditional attitudes, subjects, apparel, facial expressions, colours and media. The consequences of convention went so far as to develop a most carefully observed division of labour in the painting of Ikons: one painter specialised in gilding, another in painting dresses, another in backgrounds, another in faces, etc.[1] This specialisation carried with it a technical training of the most exclusive kind. Comparable to it, and in addition permeated with magical motives, is the initiation which was customary among the Bushmen for the training of their painters: apparently the preparation of colours was a magical guild-secret like the preparation of poisons in the same tribe[2]. A similar institution has been suggested by the Abbé Breuil in connexion with the palaeolithic paintings in the Altamira Caves[3].

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[1] This constitutes the chief interest and value of past literary and art criticism.

[2] *Vasari on Technique* (ed. with introduction and notes by G. Baldwin Brown), Dent, 1907, pp. 26 ff.

The medieval guilds—including such as the Meistersingers—though more definitely economic and artistic in essence and object, betray the same preoccupation in the maintenance of technical tradition.

B. In the development of Art from the dynamic point of view, i.e. in the history of creation and appreciation the outstanding fact is the differentiation, first, of the craftsman from the public, and later the differentiation of the artist from the craftsman.

The differentiation of the craftsman from the public is in itself a very complex process which, however, even in primitive societies appears to have at least begun, curiously enough, it seems, especially in connexion with music. I mention it as a peculiarly important development for the question before us, inasmuch as it marks the transition from manual dexterity as a *personal* qualification to dexterity as a *professional* qualification of the craftsman. The change is the outward reflexion of a psychological shifting to a point of view which is a pre-requisite to an aesthetic attitude. Once the distinction of the craftsman is secured, several other social distinctions develop of which the above mentioned guild-formations with their consequences for technical tradition and teaching are only one.

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[1] В. Н. Успенскiй: Очерки по исторiи иконописанiя. СПЪ. The painters who dealt exclusively with dresses, had the delightful technical name of "Доличные" (up-to-the-facers), p. 15.

[2] G. W. Stow, *The native races of South Africa*, 1905, p. 76.

[3] Cartailhac et Breuil, *La Caverne d'Altamira*, Monaco, 1906, p. 135.

The differentiation between the craftsman and the artist is one of the characteristic features of Art since the Renaissance. Indeed, the Greeks appear just to have reached this same point in the development of their social and aesthetic culture, when its course was interrupted by political collapse. The stage was reached again in the fifteenth century. It is, of course, true that Art has never recruited its devotees wholly from the artisan class. Both poets and to a lesser extent musicians were drawn chiefly from other sources, and it is one of the distinguishing traits of Chinese pictorial art that most of its representatives were men of letters rather than craftsmen, a fact which is reflected in the close connexion between painting and calligraphy both in China and Japan with far reaching results for the technique and aesthetics of Chinese and Japanese painting[1].

The shifting of the social status of the executant from craftsman to artist is again merely the outward symptom of a much deeper and more fundamental alteration in the relation to Art of the Middle Ages and modern times respectively. The decay of the apprenticeship-system, the rise of a new Art-education, signalised by the foundation of academies, the removal of Art from its fixed social and economic functions in medieval society to the exercise of a new, economically indetermined and ideally wholly unfettered vocation are collateral phenomena of this profound change. Psychologically and aesthetically, the nucleus of the new situation is a new conception of Art which since the Renaissance has become the basis for our modern attitude to it. Nothing could be stranger to this modern idea than the position of the medieval craftsman working to specifications of such minuteness as some of the contracts for pictures reveal, as for instance that for the Coronation of the Virgin by Enguerrand Charonton[2]. Nor could any contrast be more striking than that of a comparison between for example the *Schedula diversarum artium* of the Monk Theophilus (probably eleventh century) and the first manifesto of the modern conception: the *Della Pittura* of Leon Battista Alberti, of 1436; especially when you compare the Prologue of

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[1] R. Petrucci, *Le Kie Tseu Yuan Houa Tchouan*, *T'oung-pao*, XIII, p. 82 note; also Sei-Ichi Taki, *Three Essays on Oriental Painting*, 1910, p. 25 ff.

[2] See Thomas Okey, *History of Avignon* (Medieval Town Series), pp. 375–379.

Theophilus with the famous opening of Alberti's third book[1]. As an illuminating illustration of this new conception relevant to the present discussion, I quote Alberti's condemnation of the use of gold in paintings, from the end of the second book of his treatise:

> Truovasi chi adopera molto in sue storie oro, che stima porga maësta; non lo lodo. Et benchè dipigniesse quella Didone di Vergilio, ad cui era la pharetra d' oro, i capelli aurei nodati in oro, et la vesta purpurea cinta pur d' oro, i freni al cavallo et ogni cosa d' oro; non però ivi vorrei, punto adoperassi oro però che nei colori imitando i razzi del oro, stà più admiratione et lode al artefice[2].

The importance of the passage lies, I submit, in the fact that it records the definite loss of the intrinsic value of the medium and the renunciation of the effects of mere material preciousness in the interest of a purely aesthetic homogeneity of style.

By the end of the fifteenth century the Art of the Italian Renaissance had triumphed over the difficulties of its media and it is significant that one of its greatest representatives, Leonardo, allowed himself to be carried away by his enthusiasm for the rendering of light and of shadows so far as to remark:

> Sola la pittura si rende (cosa maravigliosa) ai contemplatori di quella per far parere rilevato e spiccato dai muri quel che non lo è, ed *i colori sol fanno onore ai maestri che li fanno, perchè in loro non si causa altra maraviglia che bellezza, la quale bellezza non è virtu del pittore, ma di quello che li ha generati*...[3].

Since then the commercial production of media has still further contributed to the loss of their practical importance. The modern painter no longer grinds his own colours and spends years in experiments with different pigments and varnishes. The present-day sculptor orders the block of marble he wants; he no longer selects and even quarries it himself as Michelangelo did. As likely as not he will leave the transfer of his model to marble or bronze to a firm of contractors.

It is true that there are two media which cannot be standardised, commercialised, contracted for. They have consequently never lost and never will lose the peculiar position which the uniqueness of a medium secures for it in artistic creation and appreciation. They are the human voice, and the human body as the vehicle of its soul. Singing and acting are the two arts which achieved their distinctive aesthetic value, the former almost from the beginnings, the latter relatively very early in the history of Art.

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[1] A concise and useful summary on several of these points will be found in the Introductory Essay of G. Baldwin Brown's edition of *Vasari on Technique*, Dent, 1907. See also the French edition of *Schedula div. art.* by Ch. de l'Escalopier with an introduction by J. M. Guichard, Paris, 1843.

[2] P. 139 of the *Della Pittura* in the edition by H. Janitschek (*Quellenschr. für Kunstgeschichte*), 1877.

[3] Quoted from the "Trattato della Pittura" in Lionello Venturi, *La Critica e l' Arte di Leonardo da Vinci*, Zanichelli, no date, but 1919.

That the growth of mechanical aids constitutes a great danger to Art which, I venture to think, has not failed to make itself felt in our times, will probably be admitted. All aesthetic psychoses are very complex states poised in a highly unstable equilibrium. This applies to the individual attitude as much as to that of whole periods, and aesthetically the functions of the medium, though subordinate to the mind of the artist, cannot be decreased beyond a certain point without the disturbance of this balance, without emasculating imagination, allowing dilettantism to usurp the place of art, and ultimately without the loss of the most precious aesthetic quality of a work: its intrinsic style.

MIND AND MEDIUM IN ART[1]. (V.)

By C. W. VALENTINE.

THE best known theories of the beautiful seem to err in the following respects: they take one aspect of an aesthetic experience, or one or more characteristics of certain selected beautiful objects, give quite convincing illustrations of these isolated factors, and then generalise as to beauty or the aesthetic experience. Very varied theories can easily arise thus; for so complex are aesthetic experiences that many varied elements may have a share in such experiences. Still more readily can we generalise if we limit our field of discussion to one of the arts.

The error mentioned above is exemplified in the theory that beauty is "the sensible appearance of the idea." True a beautiful object is often the expression of an idea: possibly it always is if we include under the term idea those 'images de traduction' to which Mr Bullough refers. But the difficulty is that beauty is usually more than the sensible expression of an idea, and that many sensible expressions of an idea, even if true, are not beautiful.

So with Croce's theory. That beauty is in some way or other an expression is doubtless true. But the theory is incomplete, as I have tried to show elsewhere[2]. The words 'intuition' or 'expression to self' (rather than to others) may cover experiences that are unworthy of being called aesthetic, though undoubtedly they do also describe one aspect of many aesthetic experiences and thus all experiences which they cover have something in common with aesthetic experience, which fact lends plausibility to the theory.

In his most interesting paper Mr Marriott has shown the importance, more especially in plastic art, of the characteristic use of the material—an aspect of art to which probably too little attention has hitherto been paid. Valuable, however, as this contribution is, Mr Marriott's thesis seems to me to err in the way described above. There is no doubt that the using of materials in a manner which is in some way suitable to their nature, is one condition of beauty in an object to those who know the nature of the material. It is still more generally true that the using of a material in a way unsuitable to its nature may spoil the object: that is, Mr Marriott's formula has wider validity in a negative form. Indeed when faced with the objection that "we can enjoy works of art without knowing how they are done," Mr Marriott himself adopts the negative form and replies that directly the means are used out of character we do become conscious of them, whether we understand their characteristic use or not.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] In a critical notice of E. P. Carritt's *Theory of Beauty* in *Mind*, 1918.

Hence to Mr Marriott's assertion that "practical and aesthetic reasons are at bottom the same thing," I would reply, "No: but suggestion of practical difficulties may spoil aesthetic appreciation." And even this is too simple a statement. For Mr Marriott's quotation from Ruskin that "In every branch of art, only so much imitation of nature is to be admitted as is consistent with the ease of the workman and the capacities of the material" must be read in conjunction with Ruskin's own doctrine that the sign of infinite painstaking, when there is adequate return, gives us one of the Seven Lamps of Architecture.

In Mr Marriott's theory there is merely one element essential to the very complex aesthetic experience. And even in considering that element he lays perhaps too much stress upon the nature of the material itself and too little upon the nature of the man who sees it. "The laws of painting," says Mr Marriott, "are as surely derived from the nature of paint and brushes as the laws of cricket are derived from the nature of bat and ball." But the laws of cricket are equally determined by the nature of man. If men acquired the power of revolving the arm ten times as fast as they can at present (without otherwise changing), either the laws of cricket or the nature of the bat and ball themselves would have to be changed. In his final paragraph, however, Mr Marriott modifies his position by applying to art Reynolds's saying about nature which comprehends under nature the "internal fabric and organisation of the human mind and imagination" and only *adding* to these words "the nature of the tools and materials used by man" in order to get a complete conception of art.

Mr Walkley criticizes Mr Marriott for externalisation, and by inquiring within leads us to Croce's Expressionism. This again, in my view, leaves us with one aspect only of the aesthetic experience. In so far as I understand Croce's 'intuition' and 'expression to self' I agree that the aesthetic experience is always of the nature of intuition: it certainly is not reasoning, or an activity devoted towards a practical end. But the term 'contemplation' or 'immediate apprehension' seems to me equally true (and inadequate). The whole world of objects and ideas may be objects of intuition equally. Croce's expressionism, so far as I can find, cannot show why one object is beautiful to us and another not, even when an object of intuition. Here again then we have a case of one quality of the aesthetic experience being lifted into the position of the sole characteristic of beauty. The ugly for Croce is incomplete expression. But what fails to be expressed in some ugly things is not clear. And why does the alteration, by a mere shade, of the tone of one colour in a design spoil the beauty? What fails to be expressed here? True I find by introspection that I can adopt, if I will, towards almost anything (e.g. even the scribble in which I am writing these words) an attitude which appears to be identical with Croce's 'intuition,' though Bullough's word 'distance' seems to me to express it better. But if we are to call *every* such mental state aesthetic, then we need quite other words for that warm experience in which we really enjoy the beautiful. I accept Croce's dictum that all aesthetic experience involves 'expression' in this sense; but not that all expression is aesthetic.

The whole question as to what is beauty, or what is the aesthetic experience, has been unnecessarily complicated, in my opinion, by the undue attention given to the activity of the artist. If it be granted that we can have a truly aesthetic experience, without ourselves creating (externally) an object of beauty, it seems unnecessary to introduce at first a discussion of the artist's creative activity in our search for a theory of the beautiful. At least it gives us an additional problem to solve, and in facing it there is usually a failure to recognise the extreme complexity of the mental processes involved. It has seemed paradoxical to deny the term 'aesthetic' to the artist's creative activity, and hence the revolt of some against Croce's description of the painter's painting as mere externalisation and practical activity. The truth seems to be that we may have in some moments of the artist's working a blend of the aesthetic with the attitude of the craftsman. In the case of the real artist the aesthetic experience is so intense, his creative imagination 'expresses to himself'—to use Crocean language—so vividly, that it endures even while he seeks to depict it with a stroke of the brush; or at least the aesthetic attitude is only momentarily effaced. This seems to be still more true of the musician extemporising at the piano. He feels for the chord to express his mind's anticipation and demand, and the technical activity is so simple and easy to him at times, that it involves no interference with the pure aesthetic enjoyment. Probably also even when the externalising activity is more prominent, it is, if successful, so enjoyable to the artist that it readily blends with the more essential aesthetic elements.

The artist's creative activity, then, and through this the influence of the medium, may enter into the aesthetic experience of the artist to a very varying degree. When the medium is relatively unfamiliar, and the exercise of technical skill becomes prominent in the whole process, the aesthetic element falls into the background and may for a time disappear. When the technique is very familiar (as in the case of an expert musician), the interference with pure aesthetic enjoyment becomes infinitesimal and the enjoyment of successful creation or interpretation adds an element of satisfaction to the whole experience which may be almost indistinguishable introspectively from the appreciation of the music itself. I should then agree or disagree with Croce's absolute distinction of the externalisation of art from aesthetic experience, according to the nature of the case as indicated above.

At this stage I may illustrate the point of complexity on which I have been laying stress, and here let us leave the artist for a moment to consider only the appreciation of a picture. In this many elements may contribute to the total enjoyment of the picture. In the course of investigations on the appreciation of pictures by large numbers of adults, it has been to me amazing to note the very different degrees to which the various elements enumerated are of influence in determining whether the picture is regarded as beautiful or not by the subject. In a given picture the following aspects may all contribute to a person's enjoyment of it—the colours used, their inherent pleasingness and their suitability for the object depicted; the individual forms and shapes; the composition or arrangement of a picture—in a good picture, as Ruskin says, "everything helps everything else"; the degree of novelty or familiarity; the significance of the picture; any associations, both conscious and subconscious, which may inevitably cling to it; its 'truth'; the perception, possibly only subconscious, of the artist's skill; and so on. Each of these factors varies in its influence according to the individual and of course according to the picture concerned. Indeed they vary according to the mood and mental attitude of the subject at the time. A mere difference in tone in the reproduction of the picture may spoil its beauty for us, a mere trivial association may subconsciously determine our attitude. The individual experience of the person concerned may make or mar the aesthetic appreciation. The Virgin Consolatrix—beautiful to one of my subjects before she had a child of her own—was intolerable to her after the birth of her child as the significance for her of the picture had been modified. In some persons the consciousness of the artist's skill contributes much more to the enjoyment of the picture than is the case with others. I believe it is especially important in the appreciation of, say, a fine portrait of an ugly person; but with many persons this aspect cannot be relied upon, and even the finest portraiture of an ugly person cannot be appreciated by them. The results of careful introspective records in the appreciation of pictures lead one to a profound conviction that the factors involved in aesthetic appreciation vary greatly according to the individuals concerned, and the same applies to the appreciation of the *elements* of music and pictorial art, as is shown in experiments with colours, lines and musical intervals. What seems to be essential for aesthetic appreciation is that some or all of the various elements should hold us entranced. This principle at least seems fundamental in aesthetic appreciation, namely that there must be at the same time stimulation and facilitation of attention[1]. "If the aesthetic experience is to be enjoyed to the full, there must be no rude interruption in the course of the mental apprehension of the object. Adequate change or complexity to maintain interest and to stimulate mental activity to a certain degree there must be. But as in a curved line there must be no sudden change entirely inconsistent with what the line as a whole pretends to be, so in a picture there must be no element which has no connection with the picture as a whole. That is the negative side of the principle. Stated positively it is that in the apprehension of a beautiful curve one part of it gives us a clue to the nature of the rest; in a good picture 'everything helps everything else.'" "In music, when the rhythm is simple, variety is supplied by the movements in the melody and the changing harmonies. In some Malay music, with an absence of melody and harmony, change is supplied by much more varied rhythms. And as in the apprehension of a beautiful curve one part gives us a clue to the nature of the rest, so the apprehension of the rhythmic form of a piece of music enables us to receive the rest of the music in a mental attitude of repose which would be impossible if the notes followed in irregular order, or if a new rhythm appeared every few bars. Music, thus, in a very marked degree satisfies the formula which one writer has offered as the definition of aesthetic experience—namely, the combining at the same time of stimulation with repose."[2]

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[1] In the case of the artist, and sometimes with others, the mind may create for itself the stimulating imaginative object.

[2] Quoted, with some verbal alterations, from the writer's *Experimental Psychology and Beauty*, 2nd edit. pp. 97 and 123.

Seen in the light of introspective records of aesthetic appreciation, the question as to whether the medium is used in a fitting or unfitting way becomes only one element among many which contribute to the total aesthetic effect; and usually, I should suppose, its influence is of that negative type to which Mr Marriott himself ultimately seems to refer it. This will especially be the case, of course, when the appreciator is relatively ignorant of the nature of the medium and of its capacities. The sight of a pastel drawing made to look like a water colour may please the uninitiated but will worry the expert in pastels. Expert knowledge of means is not only unnecessary for aesthetic appreciation, but it may cause at times a descent from the true aesthetic attitude to that of a technical expert—a cold judgment. Mr Bullough criticizes Dr Watt for asserting that a knowledge of means is unnecessary for the full appreciation of music. If by full appreciation is meant the most intense and enjoyable aesthetic experience under the influence of music, I agree with Dr Watt. But if we take the enjoyment as a whole of music, I should say that knowledge of all kinds about music (not only of means, but also of composers), by making our interest more complex, makes it possible for it to be more sustained. At least it affords moments of change from the maintainance of the pure aesthetic attitude, and so brings that back later with renewed intensity; witness the more rapid fatigue or boredom, in a long concert, of the layman as compared with the expert, though the latter may be the first to drop out of the aesthetic and into the critical attitude for a time. Furthermore, in a subconscious way, as I try to show later, the thoughts of method, skill and technique may actually coincide with a properly aesthetic experience and make it more complex.

The phrase quoted above—"stimulation with repose"—is not given as a complete description of the aesthetic experience. It covers, I think, too wide a ground. In reading an interesting yet easy book of history or of science, we may approximate to a state of 'stimulation with repose.' Yet even here we often have a state which in one important aspect resembles the full 'aesthesis,' namely, that aspect which seems to be covered by Croce's 'intuition.' But it is only the dry bones of full aesthetic enjoyment, the transition to which one may note in the variations of mood which come in reading the more argumentative passages, followed by the finest literary passages, of a great historian. Here, for the aesthetic experience to be enjoyed, we must have a contemplative and not critical or even approvingly reasoning attitude towards the subject matter. And so in the case of poetry, even for pure aesthetic appreciation it is not necessary, as Mr Marriott suggests, that the "dictionary meaning of words" must be excluded. True the appeal of the word-sounds themselves is much. But significance adds to the richness of the experience and carries us along over intervals in which the beauty of mere words fails. The old dispute as to whether substance or form is the more important in poetry, and the assertion that the poet must not try to enforce his philosophy upon us may be answered thus: that when a poet philosophizes—as is done even in some of the finest passages of literature—the reader, if he is to enjoy it aesthetically as literature, must accept, if only for the moment, the poet's point of view, so that he can simply apprehend or contemplate the philosophy and submit himself to the hypnotic influence of the poet.

And so in painting: as Dr Watt says "The arts of vision in the greatest creators have known no limitation to the purely sensuous range of data." Yes, significance counts. But it must not carry us far away from the sensuous presentation itself. The unity of the two must be very close. Or if it does lead us away, the subject matter must itself be capable of becoming a new object of aesthetic appreciation. Hence the difference between a picture which 'tells a story' of trivial moment, and one which depicts an event which itself, even as a thought, can more readily rouse the aesthetic attitude.

In conclusion I may try to sum up the main conclusion of this Symposium, though in doing so I must, I fear, give inadequate weight to the wealth of detail in the papers by Dr Watt and Mr Bullough. All Mr Marriott's successors would, I think, agree with him that in some sense an appropriate use of materials is one element that goes to make a work of art. They would maintain, however, that it is impossible to make it the sole criterion of art, as indeed Mr Marriott himself seems to admit in some passages. That the perception of beauty is dependent on the mind as well as on the object is an old platitude. Mr Walkley goes further, in admitting, with Croce, that it exists in its fullest degree in the mind of the artist before the beautiful material object is created, and apparently the mere appreciator of art must be capable of this artistic 'expression to self,' if he is to have the true aesthetic experience. This view seems to minimise the direct appeal to our feelings of sensory elements (e.g. sounds and colours), which I believe to be of essential importance in most works of art, especially in music and the visual arts.

Mr Bullough approaches to Croce's position in his assertion that even abstract thought can be the object of aesthetic appreciation, and I imagine that here he probably means something like that which I have described as the receptive contemplation of a philosophical idea, a 'simple apprehension' (in Stout's sense) as applied to an idea, instead of to a material object; and I imagine that Mr Bullough would agree with me in finding in this aspect of mental process one common trait of all aesthetic experience, however varying their sensory elements or significance may be. But I go further than Croce (and, I suppose, than Mr Walkley) would go, by asserting that often when aesthetic appreciation occurs, sense stimulation from without is absolutely essential—that is when the appreciator is quite incapable of such aesthetic 'expression' if it is not provided for him from without. He has the true aesthetic appreciation and yet is not capable of creating it, as Croce seems to teach.

Hence the medium is not only a real medium of communication between the artist's mind and the appreciator, it is a means of producing for the latter the aesthetic experience. Anything in the nature of active 'expression to self' is absent, I believe, in much appreciation of music, especially in moments in which we yield ourselves completely to the impact of the sounds. It is absent, too, sometimes in the appreciation of pictorial art, especially when pure colour is the dominant factor. It is more usually present I think in poetry. But to say that "nothing which we cannot clearly express to ourselves by actual or imagined sound, colour, word or shape or in some other way is beautiful" is, if I judge from my own experience, utterly wrong; yet I think it is a fair statement of Croce's view[1]. And even for the artist, I can scarcely believe that the richest experience is always in the creative imagination; but rather that normally, especially in visual art and in music, the artist embodies his imaginations in a medium not only to communicate them to others, as Mr Clutton Brock holds, but to make fuller and more intense his own aesthetic enjoyment. The testimony even of some poets goes to show that the impulse to write is largely dependent on a craving to enjoy more fully than can be done in mere imagination that which they cannot find in real life. "If we had life," wrote Wagner, "we should have needed no art."

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[1] The quotation is from E. P. Carritt's exposition of Croce's views in *The Theory of Beauty*, p. 182.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (I.)

BY F. C. BARTLETT AND E. M. SMITH.

1. *Statement of the problem.*
2. *The distinction between vocal and language habits.*
3. *The distinction between explicit and implicit language.*
4. *Thinking considered as a form of expression.*
5. *Thinking and its conditions.*

1. STATEMENT OF THE PROBLEM.

SINCE this Symposium is intended to take its starting point from Prof. J. B. Watson's recent discussion of "Explicit and Implicit Language Habits"[2], we will begin with a very brief statement of what seem to us to be the main points of his argument:

> 1. Thought is nothing more than a highly integrated bodily activity (page 325), chiefly identified with the action of language mechanisms (page 316); but not all thought is "laryngeal" (page 324).
>
> 2. Vocal habits are distinguished from true language habits, the former being mere repetitions of sounds, and the latter being vocal habits "associated with arm, hand and leg activities and substitutable for them" (page 319).
>
> 3. In general, language habits undergo development from overt to whispered and implicit expression: thought may be reasonably assumed always to involve implicit expression (pages 323–4).

We have to ask whether propositions 2 and 3 justify proposition 1. Let us consider one of Prof. Watson's own illustrations: "When we study implicit bodily processes we are studying *thought*; just as when we study the way a golfer stands in addressing his ball and swinging his club we are studying *golf*" (page 326). But to say that we are studying 'golf' in the second case is to assume that 'golf'—the structure and character of the game itself—is identical with how a given player *plays* golf. Similarly Prof. Watson assumes that 'thought' is, for scientific psychology, identical with its expression. This involves the central assumption of Prof. Watson's system, and raises the following questions:

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] See his *Psychology from the Standpoint of a Behaviorist*, 1919, chapter IX.

(i) Should the psychological study of thinking be confined to a determination of the varieties of thought responses in relation to their conditions?

(ii) Assuming this to be the case, can Prof. Watson's solution of the problem be regarded as satisfactory?

The second is the narrower problem and will be attacked first.

2. The Distinction Between Vocal and Language Habits.

This distinction emphasizes the fact that from the first the use of language is bound up with the substitution of one mode of response for other modes.

That both language and thinking do, in a sense, involve substitution is obvious. Both are acquirements gained during the life-time of the individual, and represent modes of response to a situation which, in the course of development, may replace earlier types of reaction. But substitution is an ambiguous word. The *mere* replacement of one set of movements by another is certainly not enough to constitute a language habit. For if it were, every conditioned reflex, for example, would be a language expression. According to Prof. Watson himself the "conditioned reflex level of functioning" gives us, at most, vocal habit; for the "true language habit" we must have this, "*plus*[1] later associative connection of the word when learned with the bodily habits connected with the object for which the word stands" (page 320). Precisely what "later associative connection" means, in this context, Prof. Watson never attempts to say.

Bearing in mind that, for the present, we are not questioning Prof. Watson's general psychological standpoint, we may attempt to work out what is really involved in the idea of substitution.

The consideration that language represents a mode of response to a situation to which earlier adjustment has been made in a different manner, will not carry us far. Not only must there be a capability of substitution, but the substituted modes of adjustment must definitely function as 'signs.'

Now if we stop short at substitution, the new modes of adjustment might be regarded as 'standing for' those alone which they have replaced. But signs involve a necessary reference to consequent adjustments, as well as a *possible* reference to those modes of responses which their present expression has replaced. A sign may be defined, from the present point of view, as any mode of reaction which is succeeded, within the total adjustment to the same situation, by other specific modes of reaction. The sign, that is to say, 'leads on to' a further series of responses, where 'leads on to' may be taken to mean 'is in the majority of cases found to be accompanied by.'

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[1] Italics ours.

But many modes of response which do not come under the head of speech are certainly signs, and, whether they are to be regarded as language or not, do not serve specifically as instruments of thinking. Any series of responses whatsoever, which are arranged in a relatively definite order, constitutes a series of signs in this sense. For example, the adjustments made by an engine driver in becoming aware of a held-up signal are signs for a further series of adjustments made by him in putting on brakes.

At this stage a further point should become clear. Supposing we specify language as a series of substitute signs; all signs are not of the same nature, and do not fulfil the same function. The most important consideration which follows is that, while some signs may have place in series in which the other adjustments are made by response of the 'special sense' organs, or of the larger muscles of the body, others lead to forms of expression for which the organs involved in the production of words alone appear to be entirely adequate. Thus to specify language as a substitution form, or even as substitute-sign forms, is inadequate.

3. THE DISTINCTION BETWEEN EXPLICIT AND IMPLICIT LANGUAGE.

Can Prof. Watson's treatment of the development of implicit language be regarded as any more complete or satisfactory than his specification of the character of the language response? He suggests that, in its growth, language makes "transition from overt to whispered and then to implicit" expression. This is not regarded as strict chronology however, for all three forms may "go on together from the start" (page 322). The two important points are, first, that practically all developed forms of language expression include responses which are of the implicit order; and, second, that particularly when we begin to study implicit expressions we are reminded that many other bodily processes besides movements of the vocal musculature may take part in expressing language (page 274).

Implicit language is simply language expression of an extremely "abbreviated, short-circuited and economized" type (page 323) and the movements it involves are excessively difficult to observe, to recognise, and to interpret. That there certainly is a tendency towards ever more and more abbreviated forms of language expression we believe every one will be prepared to agree, and also that the thinking response frequently expresses itself in these abbreviated forms.

Moreover, no doubt Prof. Watson's claim that many other bodily processes besides the vocal may play a part in the expression of thought is sound. Their occurrence is demonstrable in many instances, and probable in all. Particularly when it is remembered that Prof. Watson clearly states that he has no desire to "abstract language overt or implicit, or other implicit thought processes, from their general setting in bodily integration as a whole" (page 324), it is perfectly clear that the total thinking reaction must probably in all cases involve other than merely vocal movements. Prof. Watson candidly admits that the evidence for the existence, and function, of these 'implicit' processes is extremely incomplete (pages 326–7). For our part, however, we are willing to agree both that processes of the kind described accompany most, or perhaps all, thinking, and that their development is of importance.

4. THINKING CONSIDERED AS A FORM OF EXPRESSION.

We have already suggested there is no valid reason for holding that *all* employment of substitute signs involves thinking. A movement, or set of movements, present at any stage of the performance of a well-established habit is, from the point of view of expression, a sign, or set of signs. Both itself and subsequent movements belonging to the habit are likely to be abbreviated, and hence in a definite sense to be substituted for an original response, which was more complex and may have been different in nature. Yet we cannot agree that the simple unrolling of a habit is rightly termed thinking. Habitual response is not limited to non-vocal movements; for many of the word reactions of the ordinary man are simply habits. Catch words are repeated; the same stories are told over and over again in the same words and with the same appearance of originality. But all this is no more thinking than the repetition of a perfectly learned series of nonsense syllables is thinking. The truth is that while *all* substitute signs may properly be treated as language, some only function as expressing thought.

The thought response sign is essentially one which remains constant amid the greatest divergency of setting. From the point of view of expression, signs may certainly occur at the level of sensing, since any part of a process of sensing may bear a definite and fixed relation to other parts which precede and follow it. But sense signs are tied to immediately presented physical stimuli in the absence of which they do not occur.

Similarly signs are present in the process of perceiving but are tied to presented combinations of physical stimuli. In imaging, again, the mode of response always involves the use of substitute signs, which are frequently of an abbreviated character, and yet we rightly distinguish thinking from imaging. The fact is that the thinking response occurs, not to a physical stimulus alone, or to a combination of physical stimuli, but to some characteristic of a presented object or situation which is common to that and to many other objects or situations. Not only is it a response *to* such a characteristic, but in so far as the reaction is one of thinking, it is made precisely *as if* the characteristic were presented in an entirely different setting.

We may take one of Prof. Watson's own illustrations (page 330). "I start in to build a bridle path to the top of a hill with no special instruments and never having built a road before. If the grade is too steep I build my road around the side of the hill, taking advantage of various level places to approach the top; where the stones are too large or cannot easily be dislodged I build the road around them, finally coming out at the top. The process of constructing such a path requires manual labour almost wholly....Nevertheless, it is constructive work....In the process, though, parts of many previously organized habit systems have been used momentarily in slightly new connections. Just so in simple thought work...." Now it is not "just so," either in simple thought work or in the construction of a new road in so far as this is a relevant illustration. If it were and we inspected the road Prof. Watson had made, we should be surprised at the number of false starts to be found. It sounds perfectly simple to say, "if the grade is too steep I build my road around the side of the hill." But the real fact of the case is concealed in that statement. In so far as the response is a thought response it is definitely a response to *steepness*; not merely to a particular set of visual reactions, because that would not lead on, of itself, to the further set of muscular and other reactions involved in making the path round the hill; not merely to the steepness of *this* hill, because that also would not take me round it; but essentially to *steepness* as a quality common to this and to other situations and independent of any particular context. My construction of a path having a certain direction may, at least partially, be treated as solely the working out of habitual motor series. But what, in this instance, switches me off from the series 'going in this direction' to the series 'going in that,' is the response to a universal quality or relation. That, and that alone, gives us the peculiar characteristic of thinking.

Can we find any mode of expression specialised to the thinking response? Here we return to language and particularly to vocal language. Prof. Watson remarks that "the anatomy of the neck and upper thorax regions is the most complicated of the whole body" (page 313). Certainly vocal expression, together with its immediately associated movements, offers the most extraordinary possibility of varied combinations within which identical elements recur. Moreover, while sensory responses and manipulative responses involving the grosser muscles of the body are normally largely tied to particular forms of stimulation or to particular concrete objects, vocal response is much more readily conditioned by any stimulus or combination of stimuli whatever, and by the qualities or relations of which we have just spoken.

It may also appear as if emotional expression frequently occurs under much the same conditions. But emotional responses are, on the whole, themselves tied to particular characteristics and typical situations; the resulting bodily adjustments tend, consequently, to be more strictly limited and predictable. This we should expect in view of their close relationship to instinct. The emotional response is therefore less suited to express reactions to characteristics that must both combine with all possible settings, and remain unconstrained by any particular setting. It remains that language alone is perfectly well adapted to the expression of a reaction to universal characters in our environment.

But words may equally well constitute the response to other characters of our environment, and consequently the question arises as to whether there is any peculiarity about those word expressions which are really thinking responses. The characteristics suggested by Prof. Watson are abbreviation, short-circuiting and economizing. These no doubt apply, but they are not sufficient. With familiarity a single glance at a complicated object may do as much as, with unfamiliarity, may be effected by long and intricate manipulative processes. It is not unlikely that abbreviation is carried to a higher degree in thinking processes than in any others; but this must at best be a very indefinite criterion.

Two suggestions seem possible. First, word-thinking reactions might be catalogued, and then we could identify an instance when it occurred in any concrete setting. But this will not work; for what is in origin thinking may, in repetition, become habitual. Thus the same word response either may, or may not, indicate thinking, and there seems to be no expression difference between the words as such, which can be used to discriminate the two cases.

Secondly, Prof. Watson truly regards thinking as a very complex process; and reminds us that it must not be abstracted from its general bodily setting. Can the manner in which a certain word response is related to other reactions amid which it is found be used as a criterion of thinking? Two remarks may be hazarded. The first is that if we confine ourselves to a single occasion of thinking we can find no such peculiarity of relation. The second is that (*a*) if, taking a large number of word responses, we find that the expression in question has practically the same antecedents and consequents arranged in practically the same order, the reaction is purely automatic in character; but if the surroundings of the response, and their order of arrangement, differ, thinking may be present; and (*b*) if we *arrange* a number of instances varying in many respects but alike in some feature, or in general alike but differing in some particular aspect, we shall find that the true thinking response to these is characterized both by a minimum of delay[1] and by relatively little 'false starting.' Either (*a*) or (*b*) *might* give us a true expression criterion of a thinking process. But the test could be made reliable only after a detailed study of the conditions of the combination of habits, and it must be admitted that, at present, even at the best, we cannot, from a study of expression alone, say with more than a high degree of probability that a thinking reaction has occurred. This however is no very alarming consideration, for as a matter of fact it may be doubted whether we ever can say more.

5. THINKING AND ITS CONDITIONS.

Throughout the whole of his treatment of psychology Prof. Watson assumes that the study should be confined to a determination of the conditions of human responses, and of their adjustments. For our part we are willing to share this assumption, only remarking that it gives us no warrant for supposing, as Prof. Watson is prone to do, that problems of their conditions and variations exhaust all the important questions relevant to human responses. But this admission does not justify an identification of a response with its expression, whether the latter is overt or implicit. The danger of the reference to implicit expression becomes manifest at this point. Implicit expression only differs from explicit expression in that it is usually slighter, subtler and far more difficult to observe. It gradually undergoes attenuation until it becomes a mere assumption, whereupon it becomes easy, and appears plausible, to identify the expression with the response. We maintain, however, that the response and the expression of the response are, not only in thinking, but in all human reactions whatsoever, two different things. Throughout his discussion Prof. Watson almost invariably speaks of 'thought,' scarcely ever of 'thinking,' and in this way the distinction between response and expression appears to be slurred over. It may well be the case that whenever we think, we employ language in some form or another, but obviously this does not prove that the act of thinking and the language movement are the same.

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[1] The overt expression of thought, however, may be considerably delayed.

Can thinking, considered apart from its expression, form a subject of psychological study? Only in the most meagre manner. It might perhaps be said that thinking always involves response to relations and qualities as such, and it might even be possible to indicate to some extent its inter-relation with other processes. But any further progress would speedily demand an examination of the different modes in which thinking finds expression.

One further question remains: if we agree that psychology is, in so far as it is properly scientific, confined to a study of the conditions under which responses to a situation are made, what must we be willing to include under conditions? The answer is that we must include (1) a study of the character of the external situation, and (2) an account of the conditions which have place within the life history of the individual concerned. Among the latter, and of extreme importance in the case of thinking, are needs, wants, desires and affective processes which are all no more identical with their expression than is thinking itself.

We therefore conclude that so long as attention is confined to a study of the conditions of expression and the variations of the thinking response, Prof. Watson's treatment is broadly upon the right lines, but must be regarded as inadequately developed in respect to its details. At the same time the psychology of thinking, while it must be treated as a scientific study of the conditions and varieties of the thinking response, is only partially covered by an examination of the modes in which thought is expressed.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (II.)

By GODFREY H. THOMSON.

1. *Introduction.*
2. *Body and mind.*
3. *Thinking as substitution.*
4. *Thinking as a response to general qualities.*
5. *Thinking as trial and error by proxy, and the discovery of general qualities.*
6. *Conclusion.*

1. INTRODUCTION.

MR BARTLETT and MISS SMITH, in opening this discussion, have distinguished two general problems which are involved in the question given as title, problems which they set out in their own words on page 56, and which may be roughly paraphrased as (1) Should we be content with a Behaviourist point of view in studying thinking, and (2) If so, do we as Behaviourists agree with Professor Watson's treatment of the subject?

First, they assume for the time being the behaviourist standpoint, and on this platform they discuss how one is to know those bodily activities which 'are' thought. Professor Watson, according to them, says that the essential criterion is 'substitution.' They protest firstly that the entities in question must not be *mere* substitutes, but also signs. They point out secondly that not all 'substitute-signs' imply thought, even when these substitute signs are words. They then urge that the peculiar characteristic of thinking is that it is a response to a universal quality; and to discriminate the 'thoughtful' signs from the 'thoughtless' they ask whether the subject's response persists under different arrangements of antecedents and consequents similar to Mill's Joint Method. If so, then it implies thought; otherwise not.

For only a brief while they then return to the discussion of the behaviourist platform as such, and their opinion in this connexion appears to be that though in theory the behaviourists are restricting the psychological field, yet in practice the restriction is small.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

Now I agree that there are these two general problems involved in the question set for the Symposium, but I think they are still further separable than this. I believe the 'substitution' question can be discussed not only on the behaviourist platform but on any platform—say on a purely introspective platform. I believe the question to be, at bottom, 'Is thinking a shortened form of trial and error?' and I have no hesitation in answering it in the affirmative. The other of the two general questions is ultimately the question, 'Is there a bodily state *b* which always occurs whenever and if ever the mental state *m* occurs and never at any other time?'

2. BODY AND MIND.

Reversing the order followed by Mr Bartlett and Miss Smith, I shall take the larger question first. Obviously it is not the intention of its promoters that this Symposium should become a discussion of the Body and Mind question, and like the authors of the opening paper I shall merely state a position with regard to it.

Personally I answer this question in the affirmative; but everyone knows that any reply is an affirmation of belief, not a statement of logical conviction. Clearly, however, all behaviourists must make this affirmation, for I take it that the behaviourist creed is that the causal sequences can be followed with less chance of error on the bodily side than on the mental side (since there many examples can be studied by many, but here only the self by the self), and that theoretically all predictions which are made from mental data can also be made from physical data. At the very least the behaviourist must believe that if there are any mental states left over, then these not only never find expression, but never influence any of the other states; and this is pragmatically tantamount to denying their existence.

Provided there are no mental states thus unaccounted for, it does not seem to matter whether the relationship be one of parallelism or of interaction. This distinction only appears to enter if causation is conceived of in some anthropomorphic fashion and not as mere inevitable accompaniment.

And of course a behaviourist does not deny that mental states exist. He merely prefers to ignore them.

When Professor Watson objects[1] to making a mystery of thought (page 324) and says that it is not different from tennis playing (page 325) I agree in a fashion. I think tennis playing is just as mysterious as thought, and do not see any advantage, scientifically, in dwelling on the point; just as I think the phenomenon of Röntgen rays passing through a stone no more mysterious than that of ordinary light passing through glass. We have become familiar with glass, and with tennis playing.

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[1] *Psychology from the Standpoint of a Behaviorist*, London, 1919.

The apparent paradox is disconcerting, of thought increasing in value, intensity and complexity, as the bodily processes become more and more sketchy. But this is *because* the sketchy processes are handier to organize, and quicker: and a mathematician has no difficulty in realising x as a function increasing always inversely with a, yet suddenly disappearing (or at least becoming unreal) when a becomes zero.

The practical difficulties of the behaviourist standpoint appear to me enormous, and I do not think there is any reason for giving up introspection. No doubt it is necessary for the advance of science that a school of behaviourists should arise, who will devote themselves to the study of one side of the phenomenon and check wild statements by the other party. No doubt too their methods are essential in studying animals and in studying children. But as we approach adult psychology it seems unnecessary to avoid too strenuously the examination of our own minds.

3. THINKING AS SUBSTITUTION.

Turning to the other question, it is to be noted again that Mr Bartlett and Miss Smith, while agreeing that thinking involves substitution, do not agree that substitution necessarily involves thinking, unless 'substitution' is more carefully defined than is done by Professor Watson. The distinction, however, which they draw between a substitute which is a *mere* substitute and one which is a *sign* does not appeal to me. I cannot think that Professor Watson ever intended to speak of *mere* substitutes; I feel convinced that he means exactly what Mr Bartlett and Miss Smith mean by 'substitute-sign.' The whole point of substitution is that the substitute functions like the absentee *in a relevant way*. If one soldier goes on guard as substitute for another, he need not make up to look like him, but he must do the guard. If he looked like him but did not do the guard he would be a *mere substitute* and not a *sign*. On the other hand, if it is not a question of taking the absent soldier's place on guard, but of impersonating him so as to prove a false *alibi*, then the essential thing is that the substitute must look like the missing man. If he went on guard for him but admitted that he was Private X and not Private Y he would now (reversing the former case) be a *mere substitute*, not a *sign*. What Mr Bartlett and Miss Smith's criticism amounts to is, that they emphasize that the substitution must be relevant substitution; and to that no one can object except perhaps Professor Watson, who, unless I mistake him, will protest that he never meant anything else. His whole account shows, it seems to me, that the idea of 'leading on to' something is the essential of a 'true language habit.' See for example pp. 320–321. 'Box' is not true language when it merely accompanies the thing, but only when "the child learns that the uttered word is a sufficient stimulus to cause the attendant to hand him the objects he names without actually having to execute bodily movements with respect to them."

4. THINKING AS A RESPONSE TO GENERAL QUALITIES.

As a criterion to distinguish substitute-signs which imply thought from those which do not, Mr Bartlett and Miss Smith put forward the suggestion that thought is a response to a general quality and that it can be distinguished therefore by a kind of logical method of agreement among differences, or difference among agreements. This fails entirely to satisfy me, indeed it appears to me to be a criterion of what is *not* thought; for thought is surely not an inevitable response. I have two objections to urge, therefore. (1) The criterion would be inadequate even were it partly true. This objection, however, must I fancy be based on a misunderstanding on my part of Mr Bartlett and Miss Smith, similar to what I conceive to be their misunderstanding of 'substitution.' At all events, my objection can be made exactly on the same lines. Clearly, *mere* response to a general quality is not thinking. For a bull responds very consistently, I believe, to *redness*, even as did the roadmaker to *steepness*.

A possible reply would be that the bull does not respond to *redness* but to a red rag or a red cloak. To this I would only say that the response persists in many settings as they require, and that it is difficult to see how the redness becomes a general quality except by having a sign substituted for it; and that is Professor Watson's criterion.

(2) The criterion is erroneous. I should on the contrary say that when a response to a universal quality has become so inevitable as is suggested by the comparison with the methods of formal inductive logic, then thinking has ceased. A *habit* (on a high level no doubt) has been formed. Logical machines can be and have been made to do this part of the work. Mathematical symbols and all the machinery of mathematics are devices to short-circuit thought. Instead of the *response to* a general quality being the important thing, I should have said that the effort *to discover* a

general quality was the main point. This can be done ultimately, I believe, only by trial and error, and I prefer to look upon thought as a procedure of trial and error in which substitute-signs are used instead of actual bodily trials being made.

There is in this as in all discussion a very considerable danger of ambiguity in the use of words. Such ambiguity seems to be present here in the use of the word *habit* which is employed in a very general sense by Professor Watson, whereas it is I think more commonly used to mean what he calls a "well established" habit. No harm is done if we understand one another, and to assist such understanding is the object of the next few lines.

To Professor Watson thought is an "implicit language habit." I have just above expressed the opinion that thought ceases as habitual responses crystallize. The possibility of these differences in the use of the same word is due at bottom to the continuity of mental phenomena. When a psychological school proposes to extend the word attention to cover what is ordinarily termed inattention there is considerable justification for their action, inasmuch as there is clearly something in common all along the line from the one end to the other. Similarly with *habit*. Professor Watson protests that "many regard the term as implying something that is inevitable, invariable" (p. 269). To this one can only reply that while of course it would be wrong to restrict the word habit to acquired reactions which are absolutely invariable, yet one cannot leave out altogether the fact that the fixing of a habit means an approach to this invariability. 'Habituation' is a name for a certain direction in which responses may move, and at what point on this line of direction we begin to call the response a habit is left very indefinite. All that I wish to urge is that progress in that direction is progress away from thought, although all thought must use habits fixed at lower levels.

One other point would appear to fall under the present section. Mr Bartlett and Miss Smith say: "If we confine ourselves to a single occasion of thinking we can find no such peculiarity of relation" as will serve for a criterion of thinking. To that statement I would like to take exception on grounds of experience. Teachers, speakers, lecturers, barristers *do* to a very considerable extent know by the accompanying physical signs whether the pupil, hearer or witness is thinking, and even what he is thinking. But no doubt it is exceedingly difficult to say by what criterion one judges. Largely it is by responding to the slightest facial expressions, and to differences of very subtle order in the tone of the voice.

5. THINKING AS TRIAL AND ERROR BY PROXY AND THE DISCOVERY OF GENERAL QUALITIES.

The idea of substitution serves, to my mind, to connect thought with that method of successful response which we call 'procedure by trial and error.' Indeed, I should like, temporarily at least, to say that thought *is* trial and error, but that the trials are not complete but sketchy and abbreviated, finally being so abbreviated as to be unrecognisable. A book which was popular during the South African War was *The Defence of Duffer's Drift*, in which Lieutenant Blank, sent with his half company to defend the Drift, dreams six or seven consecutive dreams as to the dispositions he would make. On the first occasion he made a pretty camp and was scuppered next dawn. The second time he dug a trench, but a straight and easily visible one, and was again knocked out. In later trials he suffered fewer casualties but let the enemy pass. Finally he hit on the successful plan. If we imagine Lieut. Blank to have actually arrived on the ground, and to be an officer of only moderate experience, we can imagine some of the alternatives passing through his mind (*i.e.* on the bodily side implicit laryngeal and other movements taking place), and others actually leading to overt action which is presently checked. For instance, the straight trench might be begun, to be abandoned as its futility was realised. There would be, in so far as there was thought, false starts, just as Mr Bartlett and Miss Smith say.

If the officer were experienced there would be hardly any thought at all. This drift with that hill beside it and that farm at a little distance would be different from any other drift, but the general qualities of such situations would be so easily recognised and so habitually responded to, that thought would be absent except just in so far as there was difficulty in recognising them in this setting. This 'thinking' would almost certainly consist in the rejection of false starts. A sentry group which had actually been posted here would be moved thither, or it might be that the first false move never got beyond implicit action.

These false starts it is which give to any piece of written work the character of being thoughtful. It is the careful trial and rapid rejection of the alternatives which impresses the reader, who values the work the more highly, the more abbreviated these false starts are, provided their rejection is conclusive. Thinking over an essay which we are reading consists mainly in trying the paths which the writer has declared to be *culs-de-sac*, and in endeavouring to discover others.

A friend of mine who is an accomplished mathematician has more than once remarked on his changed attitude of mind now towards two lecturers under whom he sat at Cambridge. Then he thought A (whose lectures were useful and flawless) much superior to B (who floundered among mistakes on the blackboard). "Now," he says. "I realise that I learned far more from B, for in his lectures *we were seeing him think.*"

The false starts, then, appear to me to be of the essence of thinking. The rejections are due originally, no doubt, to the affective states associated with first the actual action, later with the substitute sign; so that there is a very intimate connexion between thought and feeling. The wry face which we make when we cannot solve our problem is the expression of disgust.

6. CONCLUSION.

If finally I return to the definite question 'Is thinking merely the action of language mechanisms?' then I must confess that two words stick in my throat—*merely* and *mechanisms*. If that question means what it would appear to mean to a plain man, then my answer is in the negative. But if it means, 'Is there always something going on in our body when we are thinking, which as uniquely defines our thinking as does our consciousness of thinking, and is that something especially distinguished by being, in normal persons, chiefly compounded of incipient speech expressions or impressions?'—then I can agree. I have for many years, and especially during the last few weeks, spent a good deal of time in making observations in a school for the deaf and dumb, and nothing impresses one more with the importance of language for thought. When free communication is difficult to establish with others, as it is with deaf and dumb children, it is of course very easy to say that *we* think and that *they* don't; and one must guard against such injustice. But nevertheless the conviction is forced upon one that, whatever the potentialities of the imprisoned mind—or the inaccessible brain—may be, the actual thinking is in fact tremendously hampered by lack of a mother-tongue (for these children have no mother-tongue).

As against Mr Bartlett and Miss Smith, I agree with what I imagine to be Professor Watson's criterion of language (or other 'implicit function') namely that the essential point is 'substitution.' But the substitution must be relevant substitution, and this, which I take it is what Professor Watson meant, really covers all the other points. For sooner or later in the sequence *mere* substitution will prove not to have been substitution; and the search by trial and error for the relevant point among diverse settings is the same thing as the discovery of and response to general qualities. Only I think that when the search is finished thinking has ceased, and thereafter a well established habit appears, which may itself be used in other thought. Moreover, the whole process is a continuum, and before the one level of thinking has ceased, the other has already begun.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (III.)

By T. H. PEAR.

1. *Introductory.*
2. *What is thinking?*
3. *The distinction between thinking and its expression.*
4. *The rôle of imagery in thinking.*
5. *Is the behaviourist's explanation of 'imageless thinking' adequate?*
6. *The appeal of behaviourism to certain types of mind.*
7. *Behaviourism and the problem of meaning.*

1. INTRODUCTORY.

IN considering Professor Watson's views, I have taken them from his two published books[2], and not from his later work only. For this decision there were two reasons. The first was that the 1919 statement of his beliefs has already been criticized by my three predecessors; the second, that this later account seems to bear traces of the "abbreviation, short-circuiting and economy"—to borrow his own terms—which all thought processes inevitably suffer on frequent repetition. Indeed, while reading the 1919 book, one sometimes runs the risk not only of forming language habits but even of substituting them for thinking, and of allowing to pass, in their telescoped form, some of his statements, the earlier and fuller presentation of which is far more challenging.

2. WHAT IS THINKING?

To define thinking rigidly and precisely is difficult. It may even involve the use of terms which, standing for ultimate things, are themselves incapable of definition. It is therefore probably more satisfactory to describe or to indicate common experiences which have fallen to the lot of most of us; to say "this is, and that is not, thinking," and thus to distinguish between thinking and the mere revival of experiences.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] *Behavior, an Introduction to Comparative Psychology*, New York, 1914; *Psychology from the Standpoint of a Behaviorist*, London, 1919. For the sake of brevity in the text the first of these will be referred to as "B," the second as "P."

Let us suppose that, the time of year being March and the place somewhere in the North of England, a jaded business man is told that he must take a week's holiday. Five minutes later, being found in a brown study, he explains that he is 'thinking' whether to go to Cornwall or to the Lakes. Now, such a statement may describe at least two kinds of experience, only one of which is, to my mind, genuine thinking. His 'mind's eye' may have conjured up for him first the swirling greens and purples of the sunny Atlantic, then the peaceful snow-covered Langdales: he may feel again the sun's warmth as he lazily sprawls on Mullion cliffs or the sting of the wind and the pull on his muscles as he negotiates the Pillar. Finally, the appeal of one place or the other toppling him over, he sends his telegram North or South, and our interest in him ceases.

Now, allowing oneself passively to be tossed hither or thither by competing memories until, finally, one is cast up on some shore or other, though a common way of arriving at a decision, is not, I submit, *thinking*. Yet let us suppose that our friend, realising that to him the chief appeal of Cornwall is its lazy warmth, of Cumberland its exhilarating activity, that the first involves a long day's journey, the second an afternoon's, that the one holiday will be expensive, the other cheap, remembers that he has only a week, a slender purse, and an urgent desire for violent exercise. If, then, on the grounds of time, money and health, he chooses the Lakes, he may justifiably claim that he has "thought the matter out." Now what psychological processes does this involve?

In such an experience there seem to be the following salient features:

(i) Recall of past experiences; perhaps a faithful recall, but more probably one which is the result of processes of selection, condensation and the like.

(ii) Abstraction of their relevant aspects, under the guidance of conscious or unconscious directive tendencies, of which logical thinking and the mechanism known as the 'censorship' are special examples.

(iii) Comparison of those experiences, with the discovery of their likenesses and differences.

(iv) Re-comparison, with some aim or aims in view.

(v) Combination of the results of this comparison into some conclusion which is new for the individual himself.

(vi) Expression of this conclusion to others, or to himself, in speech, writing, gesture or action.

I am inclined to believe that unless in (v) the conclusion *is* new for the individual, it cannot without ambiguity be termed a result of thinking.

Like Dr Thomson, and for the same reason, I find it difficult to put thinking and habit into the same category. When an acquaintance says "Good morning" to you, it is unwise to take this as a proof of his 'thoughtfulness,' for (or of) you; as an expression of his earnest wish that during the present forenoon everything will go well. And such caution is certainly to be advised if, as sometimes happens, his remark be made in the evening. To call such a person absent-minded surely implies our belief that in this case his language mechanism, or language habit, did *not* express his thinking.

3. THE DISTINCTION BETWEEN THINKING AND ITS EXPRESSION.

But the conclusion in (v) may be unexpressed, or only partly expressed, verbally by the thinker to himself. The thinking may be 'imageless,' and wordless. Its expression, or the way in which it issues and records itself for others, is commonly in words. But this thinking and the expression of thought are as distinguishable as skating from the figure skated. We do not refer to an '8' on the ice as skating but we call it a skated figure. And for a similar reason I cannot see why we should designate as thinking the language processes which are merely one expression of it. Like Miss Smith and Mr Bartlett, I do not agree that "when we study the way a golfer stands in addressing his ball and swinging his club we are studying golf"[1] in any complete sense of the word 'study.' We are merely observant spectators of a temporary expression, or acute symptom. Moreover, if we think that this is studying golf, are we not exemplifying perfectly the shortcomings of most lookers-on at games, and perhaps, too, at behaviour?

To me it seems that 'golf' may mean:

(1) The exhibition of the player's actions to others.

(2) The 'feel,' 'look' and 'sound' to the golfer of his own temporary activity. (For obvious reasons, I am here avoiding the terms 'introspection,' 'sensation' and 'image.')

(3) The knowledge, which may or may not express itself in action, speech or writing, of the collection of rules, customs and traditions which constitute golf.

(4) That kinaesthetic knowledge which the arrogance of our intellect sets aside from other knowledge, calling it skill.

If we watch the apparently careless ease of a first-class skater, and then inspect the figure which he has traced, are we studying 'skating'? If so, we are adopting a method of investigation which, unless supplemented, will carry us only a little way towards appreciating what the skater himself means by skating, or understanding how the geometrical simplicity of the figure is paralleled by the extraordinary physiological and psychological difficulties in cutting it. So it would seem to be with our thinking; the words which express its results to others, or even to ourselves, are often but ready-made suits which we catch up for want of better ones to hand, which clothe our thoughts but baggily and hide their individuality of outline.

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[1] P. 326.

4. The Rôle of Imagery in Thinking.

I have tried my hardest, and have failed, to find a way of discussing briefly the subject of this Symposium without mentioning the behaviourist's attack upon the doctrine of the image. And the reasons for my failure seem to be these; first, that it seems impossible, at this stage of our knowledge, to discuss thinking without mentioning imagery, secondly, that Professor Watson's account of the commonest type of imagery—visual—seems to me questionable, thirdly, that I am far from convinced that he has made out a case for the dismissal of the image from psychology. I must therefore discuss briefly some aspects of the relations between thinking and imagery.

The raw material upon which thinking operates, and without which it cannot exist, is composed of selections from revived experiences. What shall such raw material be called? I should be inclined to say that it consisted largely of *ideas*, in Professor Stout's sense of the term[1], and *notions*, in Professor Ogden's[2]. But this at once exposes me to criticism from Professor Watson, for an idea is a significant mental *image*, an image *plus* its meaning, and a notion is "a content of experience that sums up in a nuclear form a series of experiences which, *in extenso*, would involve sensations, *images* and affections." And it is just the "fiction" of images, with their aliases—"centrally aroused sensations" and the like—, which behaviourists assert must be destroyed. In 1914, Professor Watson admits that the inclusion of the image weakens the claims of the behaviourist, and adds that "it seems wisest, even at the cost of exposing the weakness of our position, to attack rather than to remain upon the defensive."[3]

The three pages which follow this statement indicate some directions along which an attack might some day be conducted, but, surely, little

[1] *Manual of Psychology*, London, 1904 (2nd edition), 410.

[2] *Introduction to General Psychology*, London, 1916, 90.

[3] B. 17.

else. And in 1919 the word image does not appear in his book, because Professor Watson finds that he can "get along without it" (p. viii). Yet in the intervening years scores of investigators have been studying thousands of cases in which the main focus of interest has centered upon what the ordinary psychologist calls images. The obsessive memories, dreams, and hallucinations of the war-neuroses—more especially of the anxiety-neuroses, where the behaviour and speech of the patient often triumphantly succeeded in concealing his thoughts—all these have flooded the literature of psychology. Few other psychologists, especially perhaps those dealing with hallucinations and dreams, seem to have rejected the conception of imagery, and neither those three pages in 1914 nor the 1919 book convince me that Professor Watson can dispense with it.

I hope it is clear that I am not here asserting or denying the existence of any metaphysical differences between image and percept, nor am I venturing any risky speculation as to whether they differ psychologically in 'texture.' But at present, being concerned merely with the province and methods of psychology, I cannot see that Professor Watson has adduced evidence sufficient to bring about the abandonment of the study of 'imagery' by 'introspection.' Moreover, the events which have occurred since his first book was published would appear to have made it obvious that the introspective approach in psychology cannot yet be abandoned. I am therefore inclined to agree with those writers who hold that behaviourists should widen their scheme in order to admit the image.

5. IS THE BEHAVIOURIST'S EXPLANATION OF 'IMAGELESS THINKING' ADEQUATE?

Let us now discuss the case in which this debatable entity, the image, becomes unimportant for the purpose of discussion or vanishes altogether; viz. that of 'imageless thought,' or, to put it more narrowly, of certain experiences termed 'awareness of meaning,' in which images are undiscoverable even by expert introspection. If such awareness of meaning be no longer 'carried by' images, even by images of words, spoken, heard or seen, this would seem to be the place for a behaviourist's theory of thinking. Such imageless awareness, leading perhaps to imageless thinking, may be 'carried by' or 'go on in terms of' implicit language, or may be identifiable with implicit language itself. I include both possibilities, for I am not always quite certain whether Professor Watson favours one or the other. He says of "'reasoning, imagery, etc."...

"such processes are dependent upon language or upon a set of similarly functioning bodily habits put on after language habits[1]"; the latter expressing the means by which we have "short-circuited (substituted for) the word system of thought[2]."

Now it is just the latter which enable Professor Watson to give a negative answer to the straight question "Do we always think in terms of words?" and thereby to escape, at least for the moment, the difficult task of explaining how it is that a man may sit for an hour unable to put his thoughts into words; how we may remember the meaning of an epigram, of a poem, or of a philosophical theory and yet be utterly unable to recall the words in which it was expressed. But of such bodily habits the examples which he gives are simple movements like the nod of affirmation, the shrug of the shoulders, or "winking, which expresses a whole series of words."

Presumably, being gestures, they eke out the inadequacy of the words. We all tend to use them, for instance, when floundering in a foreign language. But is it possible to believe that a political philosopher's awareness of the meaning of 'international relations' might consist in the mere articulation of the phrase, with or without the help of a wink, however subtle? An old-fashioned psychologist, while admitting that the articulation of the words and even the wink, the shrug or the nod might bear some of the weight of the awareness of meaning, would have shifted most of the support to the image. A less old-fashioned psychologist, believing that awareness of meaning might exist without imagery, or at any rate with the most tenuous, unsubstantial shred of it, would probably admit the participation of the words and the bodily habit. But to make them solely responsible for the awareness of the meaning of, say, the theory of relativity seems going too fast. To quote from Professor Watson: "the train of thoughts going on in your mind, according to the upholders of the image, has no adequate behaviour counterpart while it is in transit."[3] The difficult word here seems to be *adequate*. Most people would admit that many trains of thought may have adequate behaviour counterparts, but what is the really adequate behaviour counterpart to the full awareness of the meaning of $\sqrt{-1}$, unless it be the formation of the words or written symbols? But it is just this that many abstract thinkers deny. For if they be thinking in words (not in images of words, for that is not admitted by the behaviourists, but in actual word experiences), why, when they struggle for hours with

[1] B. 334.

[2] *Ibid.* 332.

[3] B. 17.

thoughts which are difficult to express, don't they just write them down and have done with it?

It would really seem that if the words are there we ought to be more conscious of them than, unfortunately, some of us are. For it seems unlikely that the behaviourist, of all persons, could wish to suppose the presence of words which are unconscious, in the sense that we have never yet been conscious of them. Yet how does he explain the common experience of waiting for a suitable epithet to drop off our pen; when we know "how we feel" about a subject, but cannot find a word? One day, in writing of a person's life work, I found myself clearly conscious not only of the aspect which I wished to describe, but also of the fact that the word in my mind—'exact'—was not the word I wanted. Next there came, to my surprise, a visual image of a flat piece of dough, upon which a ring-shaped tin cutter promptly descended, cleanly detaching from it a circular disc. Then, and not till then, my pen wrote the word for which I had been waiting,—'precise.' As far as I know, I had never before noticed the derivation of this word and its connexion with cutting. Now, if the behaviourist will allow that both the image of the pastry-cutter which came first and the word which followed, represented the meaning, we might come to terms, provided we agree that the awareness of this particular meaning, i.e. of the preciseness of the whole of a scientist's descriptive writings, was supported by, but by no means confined in, either of them.

6. THE APPEAL OF BEHAVIOURISM TO CERTAIN TYPES OF MIND.

But this leads on to another question which appears to me to be inevitable,—whether some features of Professor Watson's exposition may be expressions of his own predominant kinds of imagery. If, taking some of his phrases, the reader will study the context in which they occur: the "fiction of visual imagery"; "kinaesthetic *substitutes* for imagery," when what he is describing seem rather to be kinaesthetic *accompaniments* (B. 18); "had the idea of the image not taken such firm hold upon us..." (B. 20); that under "more constructive language types of activities" he includes the plan of a novel, the painting of a masterpiece and the composition of a great opera (P. 225); "dreams are word-reactions"; and his account of his own experiences—that before tearing off the leaf of his calendar he says aloud "tear off leaf," and that if he *thinks* the words "tear off leaf," "the words *must be uttered* silently before the habitual act arises" (B. 333); that hesitation before going to the races is expressed (in the reader, according to the text, but surely rather in Professor Watson!) in "conflicting word processes" and "that the final word act issues" (P. 332); that "in watching a fight we tend to ward off a blow or strike a blow" (P. 108); and compare them with his statement "...closer examination leads me to deny in my own case the presence of imagery in the Galtonian sense[1]," this supposition seems to be justified.

One comment upon this is that it would be interesting to know how much of this knowledge was obtained by any process other than that which psychologists usually call introspection; another that the data of introspection should be verified by observations made upon large numbers of very different types of mentality; still another, that not only has Professor Watson no right to implicate the helpless reader by his use of 'we' and 'you,' but that introspective study of imagery makes most psychologists, in writing upon such a subject, hesitate to use even the impersonal 'one.'

For to a visualiser, though this account of the "fiction of visual imagery" certainly seems stranger than fiction, it is hard to believe that it is truth. Take for instance the statement[2]: "There are probably in most cases kinaesthetic substitutes for imagery. Concurrently with the *articulated word apple*[3], there arise associated kinaesthetic impulses in eye muscles. If these latter are strong, one can see how the fiction of visual imagery might arise."

Comment has already been made upon the first sentence in this quotation. The second apparently assumes (for it is difficult to give it any other meaning) that when we image an apple we faintly articulate the word. But do all of us? The other night I said to a friend that a piece of silk needed 'oiling.' At the time I said it I knew quite well that it was crumpled, and I had a perfectly clear knowledge of the process necessary to restore its shape, viz. ironing. But if my thought of the process of ironing included the "faintly articulated word" 'ironing,' or other "kinaesthetic substitutes for imagery," presumably the articulation of the word and the kinaesthetic substitutes which correspond to ironing should have prevented the mistake.

Concerning the third sentence of the last quoted statement, there seem to be two difficulties:

(1) The consciousness of the movements of the eyes necessary to fixate an apple, or even to follow its outline, is one type of mental process, that of its colours as imaged appears to be quite another.

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[1] *Psychol. Rev.* 1913, xx. 173.

[2] B. 18.

[3] Italics in text.

(2) What little experimental (behaviouristic) evidence we have seems to be against the probability that there is any close connexion even between the eye-movements and the shape of an object, though Professor Watson says (P. 324) "we have learned to draw objects and to *trace them with the eyes*." This sentence seems to contradict the findings of experimental aesthetics[1].

To sum up this section, I cannot help feeling that the above account may be true of the 'motor-minded' person whose visual imagery is weak, but that it should be supplemented by accounts from the large numbers of persons whose powers have been bestowed in the inverse relation. It would be leading this discussion too far from its subject to speculate to what extent the extrovert type of mind tends to welcome behaviourism, the introvert to consider it inadequate. But a discussion between Professor Watson and Mr Henry James, with, perhaps, Professor William James acting as interpreter, would have been invaluable.

7. BEHAVIOURISM AND THE PROBLEM OF MEANING.

As I have indicated before, I cannot make up my mind whether the behaviourist really claims that the awareness of meaning is 'carried by' (or 'goes on in terms of') these language processes, or is identifiable with them. In either case it would be interesting to read a behaviouristic explanation of those experimental investigations of thinking which make it difficult to harmonize the laws which govern the memory of meanings with those which govern the memory of words[2]; why we can learn fifteen pairs of similar meanings in one repetition, each meaning being expressed in perhaps five words, while no such happy facility is displayed in learning paired vocabularies; why two meanings may be associated with the greatest ease although the words which express them have never been in consciousness together or even in immediate succession; why the second 'half-thoughts' in a series will at once associate themselves, and retroactively, with their partners in a series read some time before; why the subject may give the right thought in the wrong words. I cannot yet understand all this on the supposition that thinking consists merely in the action of language mechanisms.

When we recall Professor James's description of thought as a series of flights and perchings, it seems that the behaviourist has given us an account of some kinds of perchings, and, fascinating as it is, it reads like a description of flying by an aerodrome mechanic, who sees only the last stages of the aviator's descent. But, to the upholders of the image, as well as to the behaviourist, the 'awareness of meaning' still remains an unsolved problem. Even if it turns out to be incipient behaviour, there will, probably, still be room for its study, from the inside, by introspection.

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[1] See Valentine, *The Psychology of Beauty*, London, 44 f.

[2] Cf. Külpe, "Ueber die moderne Psychologie des Denkens," *Internationale Monatsschrift* series, and the work of the Wurzburg school generally.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (IV.)

BY ARTHUR ROBINSON.

1. *The general programme of behaviourism.*
2. *Thinking and language mechanism.*

1. THE GENERAL PROGRAMME OF BEHAVIOURISM.

THE question set as the subject of this Symposium arises directly and inevitably from the programme of the Behaviourist school in psychology, and it is difficult or impossible to discuss the issue raised without briefly considering certain features in the general situation. This is happily rendered all the easier by the circumstance that we are dealing here and now chiefly with the views of Professor Watson, who (unlike some others) does not concern himself with any matters usually called philosophical, but seeks a straight scientific solution of a perfectly definite problem. As a quite necessary preliminary the 'traditional' or 'classic' psychology is quietly moved out of the path of progress. The business of psychology is the prediction and control of behaviour. Adjustment to environment is assumed as the characteristic situation; in adjustment we have a response and a stimulus or situation which calls out or determines that response. Hence "the goal of psychological study is the ascertaining of such data and laws that, given the stimulus, psychology can predict what the response will be; or, on the other hand, given the response, it can specify the nature of the effective stimulus."[2] The terms *stimulus* and *response* are used in their physiological sense, but *stimulus* is also used to include a *situation* (*i.e.* a complex group of stimuli), and *response* to include several complex simultaneous responses (*i.e.* adjustment)[3]. The crucial divergence from the current view lies in this, that Professor Watson holds it irrelevant to inquire whether, when an organism responds, the stimulus responded to is in its field of consciousness or not. It makes no difference, from this point of view, whether the organism does or does not know what it is doing. All introspective material is left outside the inquiry, though introspection is rejected not on speculative but on practical grounds. Progress is held to be impossible unless the introspective method is discarded. "This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier which exists between psychology and the other sciences. The findings of psychology become the functional correlates of structure and lend themselves to explanation in physico-chemical terms."[1]

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] B. 10. I follow the notation of Prof. Pear, B. referring to Prof. Watson's *Behavior, an Introduction to Comparative Psychology*, New York, 1914, P. to his *Psychology from the Standpoint of the Behaviorist*, London, 1919.

[3] P. 10, 11.

The upshot of course is that psychology is in no sense a science of mind. We have long been accustomed and hardened to the conception of a psychology without a soul, but a psychology without a mind does seem—*sit venia verbo*—'the limit.' Of course it would be idle to make much of the mere name, and plead the established use and sense of the term psychology. We may agree with Professor Watson that after all the name is not of much consequence. The fundamental point is to know the exact question to which your science is to find an answer; a science is defined by its problem not by the derivation of its name. Let us assume for the moment that adjustment and its conditions furnish a sufficient frame for what we need know about man and his actions, yet Behaviourism, by restricting itself to the physiological conception of stimulus and response in its premises, is quite obviously restricted to the same conception in its conclusions. That is to say, its findings will be about the body not about the mind. My point is that although Behaviourism is clearly right in leaving out of consideration any data which can be proved to be irrelevant to its problem, no one doubts that man is conscious, and this fact, which at least appears to be relevant to his actions, ought not to be discarded without some evidence of its irrelevancy. Such evidence has yet to be produced.

Psychology, then, according to this theory is to study behaviour only, and behaviour is the adjustment of the body to its environment. *Explicit* behaviour "involves the larger musculature in a way plainly apparent to direct observation." *Implicit* behaviour involves "only the speech mechanisms (or the larger musculature in a minimal way; *e.g.* bodily attitudes or sets)."[2]

Professor Watson naturally does not fail to see that to complete his scheme images and affective processes must be disposed of. The former are held to be non-existent or problematic; the place of the latter, it is suggested, may be supplied by physiological processes—changes in secretion, circulation, etc.[1]

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[1] B. 28.

[2] B. 19.

The programme is now in outline fairly complete. Consciousness plays no part in it except the part it plays in relation to all science—that is, it is external to, and absolutely non-constitutive of, its object—or, in other words, it means the awareness of the investigator. All the same, Professor Watson believes that "Psychology as behaviour will, after all, have to neglect but few of the really essential problems with which psychology as an introspective science now concerns itself."[2]

The answer to the question raised in this Symposium, it seems clear, must depend on the acceptance or refusal of the conception of Psychology which has just been outlined. This conception I am unable to accept for the following reasons in chief:

It seems to me quite unjustifiable to rule out data simply because they are 'private' and are therefore less easy to verify than 'public' data. The circumstance is no doubt a disadvantage for scientific investigation, but the data are there, and the universe does not seem to have been constructed with a too careful eye to the convenience of those curious enough to try to understand it. Introspection, therefore, I feel compelled to retain, though I mean by it nothing more mystic than the fact that I can have a headache and know it. Now it is clear that by limiting the subject matter of psychology to bodily states and processes, to physico-chemical changes, the Behaviourist does not solve the problem which the traditional Psychology attacked but substitutes another (though a possibly connected one) for it. The old question is, What processes form the contents of a field of consciousness, what is their nature, what their laws, and how are they related to the bodily processes which accompany them? The Behaviourist asks: To what physical stimulus is a given physico-chemical change in the body a response? In a word the traditional psychologist studies a psychical system to which consciousness is internal, and whose constituents are sensations, images, emotions, etc., the Behaviourist a system simply physical.

The starting-place of psychology is the enquiry: What is present in the field of consciousness when an organism makes a given response? Man is like a machine with an inner dial, we want to know whether the reading of the introspective indicator agrees or disagrees with the direction which the organism is overtly taking. Nor can I help thinking that this information is no less essential than the behaviour of arm, leg, or larynx. When a man insures his life, is his response governed in any way by prevision of an event possibly far distant, by the conscious recollection of the unwisdom of others, etc., or can his every movement be linked to a purely physical antecedent or set of antecedents? If the latter be the case it would still be the most interesting point conceivable to know why or how the index of his consciousness points to quite a different nexus of conditions.

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[1] B. 26.

[2] B. 28.

I agree with Professor Pear in his condemnation of the wholesale rejection of imagery by Professor Watson and his wing of the Behaviourists. There is, as Professor Pear points out, little or no ground for it. Moreover, on any reasonable estimate of the Freudian movement nothing is clearer than the immense importance and significance of the play of imagery in dreams, fantasy, and indeed in the whole field of human adjustment. If the business of psychology is the prediction and control of human action, is it not sheer folly for the Behaviourist to limit himself to only one side of the evidence, and that by no means the most significant?

The treatment of affective processes is also unfortunate, for affection is certainly not identical with changes in circulation and glandular activity. Yet in their reaction against psycho-physical parallelism the Behaviourists rush straight on one of the worst points of that theory. Professor Thomson says the Behaviourists do not deny that conscious processes exist; they only prefer to ignore them. But they can only ignore them on one of two suppositions. They must suppose either (*a*) that conscious processes are inefficient, or (*b*) that they are so 'expressed' in bodily processes that the mental can be adequately studied in its bodily outcome. On the first supposition, all that we commonly call mental—desires, hopes, fears, imagination, reasoning—can have had no effect whatever on the process of evolution; the field of consciousness has not only been without effect: it has not even served as a mere index of the conditions to which the body has responded, but has been simply an uncalled for and inexplicable illumination of a chain of events to which it has no relation that can be intelligibly expressed. The second supposition is definitely rejected by Professor Watson. One of the chief points of his programme is to elevate (or reduce) the study of human to the same level as the study of animal psychology, in which the agent's 'mind' is inaccessible. The bodily processes are therefore not to be studied as the 'expressions' of anything. "It is granted that the behaviour of animals can be investigated without appeal to consciousness. Heretofore the view-point has been that such data have value only in so far as they can be interpreted by analogy in terms of consciousness.

"The position is taken here that the behaviour of man and the behaviour of animals must be considered on the same plane; as being equally essential to a general understanding of behaviour. It can dispense with consciousness in a psychological sense. The separate observation of 'states of consciousness' is, on this assumption, no more a part of the task of the psychologist than of the physicist."[1]

On the general issue, therefore, I answer that the Behaviourist statement (*a*) mutilates the essential problem of psychology; (*b*) neglects evidence which exists, is accessible and relevant to the understanding of human nature; (*c*) leaves the undoubted fact of consciousness an isolated and gratuitous miracle.

2. THINKING AND LANGUAGE MECHANISM.

As to the special question of thinking regarded as the action of language mechanism, my answer follows quite inevitably from the position taken in the previous section. Muscle and gland activity is not thinking, and nothing whatever can be gained by so regarding it; we have seen that the Behaviourist does not welcome the suggestion that the bodily activity may be treated as the 'expression' of the mental.

Mr Bartlett and Miss Smith quite rightly draw attention to the ambiguities of the terms 'substitute' and 'substitution' in the Behaviourist theory of thought. It is quite clear that if we keep strictly within the abstractions of that theory it becomes exceedingly difficult to give an intelligible account of language. Consider Professor Watson's example, which I quote at length to avoid the possibility of injustice in a summary statement.

"A stimulus to which a child often responds by opening and closing and putting objects into it is the box in which his toys are kept. The nurse, observing that the child reacts with his hands and fingers to the box begins at a suitable age to say 'box' when the child is handed the box, 'open box' when the child opens it, and 'close box' when he closes it and 'put blocks in box' when that act is executed. This is repeated over and over again until the conditioned reflexes are thoroughly established. In the course of time the box in front of him, which originally called out only bodily habits, now begins to call out word habits. He says 'box' when it is handed to him, 'open box' when he opens it. The sight of the box now becomes a stimulus capable of releasing either the bodily acts or the word acts, or both. A series of functional connections is established between vision and throat and laryngeal muscles which exists alongside of the already earlier established connections which run from the same receptor to the arm and leg muscles. When the box is presented now, which act will take place? Manual action or laryngeal action? It is at this point that the influence of the environment upon the shaping and forcing of language habits comes clearly to the front. There comes an occasion when the box can be seen but not reached. Action in the hand is blocked. He speaks 'box' and may speak it persistently all over the house. The attendant, hearing the word box, hastens to put it in his hands. In view of the fact that this situation is repeated day in and day out, not only with respect to this object but to hundreds of others, the child learns that the uttered word is a sufficient stimulus to cause the attendant to hand him the objects he names without actually having to execute bodily movements with respect to them. There has been a substitution of the language habit for a bodily habit—now by a word he can cause adults to move—his grunt or growl or infant toy word is law."[1]

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[1] B. 27.

On the ordinary view, which admits the appeal to the consciousness of the agent, the child has learnt that the word 'means' the thing, and the utterance of the word becomes an instrument in the service of a conative trend. But the Behaviourist has no business to know that the child 'desires' or 'wants' the box. The consequence is, that if the Behaviourist is to be faithful to his initial abstractions and keep within the limits he has laid down, he must define his substitute-response simply by the place it occupies in the general 'pattern' formed by all responses implicit and explicit. But what is there in the nature of a laryngeal response as such which can possibly label it as a substitute for a response of 'the larger musculature'? The only differentiating circumstances possible are order in time and frequency of accompaniment, and their insufficiency is too patent to need comment. Consequently the notion of substitution does not seem to work, and it is by no means clear what 'relevancy' could mean in the theory under discussion.

Similarly thinking could not be regarded as a case of trial and error. Error surely means that the agent does not get what he wants, and appetition and conation are outside the Behaviourist scheme of things The Behaviourist is attacking his problem by the aid of only one eye and one hand, having two eyes and two hands at his service.

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[1] P. 320–1.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (V.)

BY JOHN B. WATSON[2].

1. *A correction of statement.*
2. *More comprehensive use of the term 'thinking' demanded.*
3. *Illustration of thinking made overt.*
4. *Behaviourist's right to assume that a process of implicit thinking goes on.*
5. *Further elaboration of the process of thinking; some objections reviewed.*
6. *'Conceptual' thinking really a fallacy.*
7. *'Meaning' an experimental problem and not a problem of philosophy or of speculative psychology.*
8. *Conclusions.*

1. A CORRECTION OF STATEMENT.

BEFORE attempting to define further in this Symposium the behaviourist's position on thinking, it would seem best to discuss for a moment some of the statements the behaviourist has already made. In advance of any argument I think we can say that he has never really held the view that thinking is merely the action of language mechanisms. Possibly my own loose way of writing may have lent colour to such a view. I frankly admit that in a number of paragraphs in a recent book I may justly be accused of having given an affirmative answer to the question before the Symposium. I can make only the well-worn excuse that my over-emphasis was indulged in for the sake of sharpness of presentation before elementary students.

In psychology we can rarely make a complete observation of everything that a human being does at any one moment; and in giving an account of what happens we emphasize those points which the experiment was designed best to bring out. This is what I meant to do in my previous discussion of thinking. I gladly emend any statement I may hitherto have made as follows.—A whole man thinks with his whole body in each and in every part. If he is mutilated or if his organs are defective or lacking, he thinks with the remaining parts left in his care: but surely he does everything else in exactly the same way. If one studies a game of tennis, one's observation is taken up with the type of strokes the player makes, his serve, his returns, the way he covers the court, etc. In other words, arm and leg activities are principally emphasized. However, everyone admits that the player is using every cell in his body during the game. Nevertheless if we sever a small group of muscles in his right arm his playing is reduced practically to that of a novice[1]. This illustration serves us very well in explaining why one emphasizes laryngeal processes in thinking. Surely we know that the deaf and dumb use no such laryngeal processes, nor does the individual whose larynx has been removed. Other bodily processes have to take on the function of the larynx. Such functions are usually usurped by the fingers, hands, arms, facial muscles, muscles of the head, etc. I have in another place emphasized the extent to which finger and hand movements are used by the deaf and dumb when they are engaged in silent thinking. Mr Thomson in his paper before the Symposium seems more or less to subscribe to the same view. It would be an easy experiment, but so far as I know not hitherto tried, to bind the fingers and arms of such an individual and then give him a problem in arithmetic, memorizing simple stanzas, and the like, which have to be worked out without exteroceptive aid. It would be necessary probably to tie down eye movements, were such a thing possible, and to restrain even the head and intercostal muscles.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] The writer had not the opportunity of seeing the preceding contribution to this Symposium.

While there is no sacrosanct reason why thinking should go on in normal individuals in the muscular fields of the larynx and throat, there are two very practical ones. There is first the genetic fact that, from childhood onwards, organization has been forced in the direction of language activity. From the third or fourth year onwards probably a thousand language adjustments are made to one manual adjustment. There is, too, a biological reason. This arises from the fact that the human being in his early struggles for existence had to have the undivided use of arm, finger and hand musculature when hunting and fighting. If he had had to employ the large muscles in thinking as, I am convinced, deaf mutes do, manual activity would have been interfered with at critical times. I have never had the opportunity of observing deaf mutes in a fight or in a critical situation where both thinking and delicate action of the manual type were demanded.

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[1] There remains to the player of course a large organization—his verbal organization especially is left intact as is the training his eyes have received. He is able to act as umpire, to pass judgment upon playing, write manuals on the structure of the game, etc. Probably he could learn to play with his left hand much more rapidly than a poor player who had suffered a similar loss, in view of the fact that his leg and trunk organization is relatively intact.

2. MORE COMPREHENSIVE USE OF THE TERM 'THINKING' DEMANDED.

The question is often asked what marks off thinking from the mere subvocal unwinding of well-organized language habits. Mr Bartlett and Miss Smith have brought out this question explicitly and have formulated an answer which is not satisfactory to me. I think we ought to make the term 'thinking' cover generally all implicit language activity and other activity substitutable for language activity. [It should be admitted furthermore that under proper stimulation (usually a request is sufficient) the subject can be made to think aloud.] Thinking would comprise then the subvocal use of any language or related material whatever, such as the implicit repetition of poetry, day dreaming, rephrasing word processes in logical terms, running over the day's events verbally, as well as implicit planning for the morrow and the verbal working out of difficult life situations. The term 'verbal' here must be made broad enough to cover processes substitutable for verbal activity, such as the shrug of the shoulder and the lifting of the brows. It must embrace the implicit movements involved in written words or the implicit movements demanded in the use of the deaf-and-dumb sign manual, which are, in essence, word activity. Thinking then might become our general term to cover all subvocal behaviour. It is obvious that this definition can take care of the most mechanical and deeply grounded of our language habits such as those used in the subvocal repetition of childhood verse, the repetition of stanzas of poetry, limericks, etc.; those depending more particularly upon emotional stimuli as day dreaming, as well as those verbal processes not completely habitual such as the working out of a lecture, the planning of a book; and finally those in which new results are brought out. It is clear that if in the interests of systematic psychology we need to sub-divide the whole process of thinking; three lines of cleavage will at once appear.

1. Mere unwinding of vocal habits where the word sequences are invariable: illustrated by hymes, quotations; by many of the responses in mathematics, as 2 and 2 equal 4, square root of 9 equals 3, and the like. Here there is no new work, no trial movements like those we see

in overt manual activity when a new situation capable of solution is presented the first few times. Such thinking corresponds to an extremely simple stimulus and response type of behaviour. Similarly day dreaming would fall under this division. We assume that such dreaming takes place in response particularly to deficiency stimuli of one kind or another; such as the absence of sex activity, lack of food and water, lack of habitual surroundings and companions, lack of drugs, or even under the sway of drugs.

2. The solving of problems which are not new, but which are so infrequently met with that trial verbal behaviour is demanded; illustrated probably by thinking out of stanzas, partially forgotten; in trying to apply one mathematical formula after another in a particular problem at hand. All of the part processes have been met with by the individual and are part of his organization, but he cannot use these part processes with machine-like facility.

3. Finally we have the extreme extension of 2 above. Here the problem is new and the organism when confronting such a problem is in a grave situation. We will suppose, for example, that a man loses his position and wealth suddenly and must be ready in a few hours to act explicitly in a new undertaking. The problem, it is assumed, is of such a character that it must be worked out verbally before any overt action can take place. Hundreds of examples of this type immediately suggest themselves. Most of the real social and moral problems appearing in one's life are exactly of this type[1].

These subdivisions are really guesses as to what may go on. No scientific division is as yet possible. It should be expressly stated, furthermore, that thinking in any of the above forms is not an isolated process. A human animal never gets away from his biography; and the varying organic and emotional states the organism is in must exert a tremendous influence upon the course of his thinking. So that once more we would emphasize the fact that thinking, whatever its type, is an integrated bodily process.

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[1] I am rather startled by the fact that all of the writers in the Symposium seem to find some confusion in my use of the term 'habit.' They maintain that I apparently imply by the term a fixed or invariable chain of responses. Of course there are a few such invariable sequences in every human being but the number is not large. When used in this sense I quite agree with them that it is opposed to thinking if we mean by thinking the solving of problems such as those indicated in my third division. I have generally made the term habit coextensive with that part of an individual's organism which is not hereditary; but surely in all learning there is a display of previous organization—of habits (and hereditary activity) most closely connected with the type of situation confronting the learner. No single response already learned (habit) will bring about the present adjustment—there must be a recombination. But the partial habits forming the new whole adjustments, be they laryngeal or manual, have each had a history and their origin can often be traced.

Probably not many of my colleagues would include 1 and 2 under the term 'thinking.' Thinking has come to be identified with 3 of our division, but for no valid reason. We use the term manual activity when our subject ties his shoe strings in exactly the way we use it when he is *learning* to manipulate (for the first time) the most complicated of machine-gun mechanisms. In our opinion 3 represents a bit of behaviour on the part of the human animal which, when stripped of its unessentials, is exactly like that bit of behaviour which the rat exhibits when put into a complicated maze for the first time. When it gets to the food the autonomic strivings die down and it goes to sleep. The deficiency stimuli, lack of food, lack of usual surroundings, etc., cease to operate—the adjustment is complete. Surely a similar thing takes place in man. He works verbally (that is particularly verbally; many other processes go on of course, such as wrinkling the brow, tearing the hair, etc.) until certain verbal acts ('conclusions') are executed. If, when this conclusion is reached, the driving stimuli (verbal, autonomic, emotional, etc.) cease to operate, the adjustment has been completed.

3. ILLUSTRATION OF THINKING MADE OVERT.

The present writer has often felt that a good deal more can be learned about the psychology of thinking by making subjects think aloud about definite problems, than by trusting to the unscientific method of introspection. Usually a scientific man is quite willing to enter into the experiment with zest. If I ask my subject in 1 (see page 90) to think aloud he overtly responds with his limerick, his day dreaming or his mathematical answer. Similarly if I ask him to think aloud in 2, I notice hesitations here and there, false starts and occasional returns, but in general a fairly ready response occurs with relatively few errors. It is only when we ask him to think aloud in 3 above that we begin to grasp how relatively crude is the process of thinking. Here we see typified all of the errors made by the rat in the maze: false starts appear; emotional factors show themselves, such as the hanging of the head and possibly even blushing when a false scent is followed up. The subject returns again and again to his starting point as shown by his asking, "You say the given facts are so and so?" The experimenter says "Yes" and again the subject starts off. In conducting an experiment of this kind, one has to be careful to impose problems upon his subject which are as far as possible removed from repressed emotional factors. It is never possible of course completely to do this as the analysts have more than once pointed out. The following illustration will make clear some of the points which appear in overt thinking.

A colleague of mine came on a visit to stay in an apartment in which I had rooms. In a passage leading from the shower bath was a peculiar piece of apparatus standing near a sink. The essential features were a curved shallow nickel pan about twelve inches wide by twenty inches long; at one end the pan had been bent in in the form of a half circle, while at the other end the side pieces did not extend for the full width. The pan was mounted on a stand adjustable in height. Furthermore the pan itself was attached to the stand by a ball and socket joint. My friend had never seen anything like it and asked me what in the world it was. I told him I was writing a paper on thinking and pleaded with him to think his problem out aloud. He entered into the experiment in the proper spirit. I shall not record all of his false starts and returns but I will sketch a few of them. "The thing looks a little like an invalid's table, but it is not heavy, the pan is curved, it has side pieces and is attached with a ball and socket joint. It would never hold a tray full of dishes (*cul de sac*). The thing (return to starting point) looks like some of the failures of an inventor. I wonder if the landlord is an inventor. No, you told me he was a porter in one of the big banks down town. The fellow is as big as a house and looks more like a prize-fighter than a mechanician; those paws of his would never do the work demanded of an inventor" (blank wall again). This was as far as we got on the first day. On the second morning we got no nearer the solution. On the second night we talked over the way the porter and his wife lived, and the subject wondered how a man earning not more than $150 per month could live as our landlord did. I told him that the wife was a hair-dresser and earned about eight dollars per day herself. Then I asked him if he did not see the sign 'Hair-Dresser' on the door as we entered. The next morning after coming from his bath he said, "I saw that infernal thing again" (original starting point). "It must be something to use in washing or weighing the baby—but they have no baby (*cul de sac* again). The thing is curved at one end so that it would just fit a person's neck. Ah! I have it! The curve does fit the neck. The woman you say is a hair-dresser and the pan goes against the neck and the hair is spread out over it." This was the correct conclusion. Upon reaching it there was a smile, a sigh and an immediate turn to something else (the equivalent of obtaining food after search).

4. BEHAVIOURIST'S RIGHT TO ASSUME THAT A PROCESS OF IMPLICIT THINKING GOES ON.

Notwithstanding the fact that we can make our subjects think aloud and thereby can observe a large part of the process of thinking, Titchener some years ago raised against an early paper of mine the objection: "How does the behaviourist know there is any such process as thinking since he cannot directly observe it?" Titchener kindly answered this question, to the effect that the behaviourist—*quâ* behaviourist—doesn't know that there is any such thing as thinking. The introspectionist claims that the behaviourist first uses the good old-fashioned method of introspection to find thinking and having once found it shuts his eyes and turns his back upon his original method and begins to externalise the process and to put it in the universal language of science. In other words, he describes it merely as the functioning of laryngeal or other motor processes.

Before coming to closer terms with this question, the behaviourist would like to posit the assumption, without discussing its many metaphysical implications, that in no physical or biological science is the fact called into question that the investigator can make an observation; for example, that he can note that his galvanometer needle has swung two degrees to the right, that when sodium is burned on the end of a glass rod the bright visual stimulus in the spectroscope will be located on the scale at $5800\mu\mu$: that the physiologist can observe that when such and such a thing is done to an animal whose heart rate is being recorded the rate has decreased or increased. He can also make the same observations on the changes in his own heart rate due to the use of different types of drugs. He can do this either by counting his own pulse or better by attaching himself to some form of recording device. In each of these sciences the observer goes on in his care-free way, accumulating a series of systematic observations. He does not do this in any hit-or-miss way. A definite stimulus starts him upon his work—the words of the professor over him, or the written or spoken word of an antagonist, or finally some inward organization exerts its pressure. He works, for example, with the effects of strychnine upon human or animal organisms, because he has had some initial stimulus to drive him to that work. Once started, the changing results he obtains serve as a stimulus for further work. Finally he groups his facts and a bit of organized science is the result, namely a monograph upon the effects of strychnine upon living organisms. If you ask him, or the physicist who has worked up a monograph in a wholly similar way upon the spectroscopic analysis of certain compound substances, "Did you realise that there was an observer implied during all your manipulations?" he would probably not know what you meant and he would certainly be mildly angered if you happened to interfere during his working moments with such a question. In other words he gets along without discussing or even being interested in the fact that there is an implied observer at every moment in science and that a thousand interesting metaphysical points lie behind an individual's ability to make observations.

The behaviourist likewise shuts his eyes to the same metaphysical question and asks only to be allowed to make observations upon what his subjects are doing under given stimulating conditions. On the metaphysical side he asks merely to be put into the same basket with other natural scientists. The introspectionist has never made this plea to the metaphysician. He has assumed that the question of the observer is a psychological one and that he has the answer to it. The behaviourist is not so bold. He is engaged in studying, among other things, the process of observing as it appears in others, where the activity is not complicated by the demands of introspection. He must, as must the introspectionist also, assume that his own process of observing is the same as that of the subject whom he is studying. He hopes ultimately to give an adequate account of the process in this subject, an account which will show how even those phenomena which the introspectionist describes as his 'consciousness' result from the complexities of behaviour.

The introspectionist hopes for a solution of the metaphysical problem through some mystic self knowledge. The behaviourist believes in no such transcendental human power. He himself is only a complex of reacting systems and must be content to carry out his analysis with the same tools which he observes his subject using. I cannot, therefore, agree with Mr Thomson that there is a mind-body problem in behaviourism. It is a serious misunderstanding of the behaviouristic position to say, as Mr Thomson does—"And of course a behaviourist does not deny that mental states exist. He merely prefers to ignore them." He 'ignores' them in the same sense that chemistry ignores alchemy, astronomy horoscopy, and psychology telepathy and psychic manifestations. The behaviourist does not concern himself with them because as the stream of his science broadens and deepens such older concepts are sucked under, never to reappear.

Granting then that the behaviourist is a natural scientist and makes his observations upon his fellow man rather than upon himself, utilising the aid of instruments whenever possible or necessary, like any other scientist—how does he arrive at the concept of implicit thinking? The answer is that he can *at present* arrive at it only by making use of a logical inference. In those cases where the response to the stimulus is not immediate but where it finally occurs in some form of explicit verbal or manual behaviour, it is safe to say that something does go on, and that that something is surely not different in essence from that which goes on when his behaviour is explicit. Let us glance for a moment at a manual illustration. I hand a friend a gold cigarette case which can be opened only by pressing a secret spring. I tell him that he can keep the case if he can open it without violence. I watch him for two minutes, noting his rambling trial manipulatory movements. He fails to open it in this period of time. I then place him in a room alone, and tell him to come out when he has opened it. At the end of thirty minutes he emerges smiling and with the case open. Since there are no marks of violence on the case, the behaviourist, utilising logic, has a right to assume that the subject continued to work at the problem as he had been trained to work at such problems and that his behaviour in the empty room was essentially the same as that exhibited by him when he was under direct observation. Merely because observation of his behaviour could not take place so long as he was hidden from the observer gives no one the right to assume that any different or unusual process went on. I should not hesitate to call this behaviour on the part of our subject manual thinking or non-language thinking. There is no necessity for it, however, since our categories of trial-and-error learning, functioning of habit, etc. are adequate. I suggest manual thinking here to show its complete homology with that type of behaviour described below which is more universally called thinking.

Suppose instead of giving him a problem which can be learned by manual trial-and-error manipulation I say, "What would be the result on your social and vocational life if through some accident you suddenly had both arms removed?" Assuming, as would be safe in most instances, that such a problem had not hitherto been faced and formulated, he would be unable to give any adequate statement. Suppose we insisted upon a formulation. At the end of an hour he would probably be able to return a fairly comprehensive reply. Surely I have the right to assume, even as a 'despised' behaviourist, that implicit language activity, sensori-motor in character, has been taking place during the hour on as grand a scale as overt bodily movements would have been taking place had I left him in a room from which there was no obvious exit and suddenly yelled "Fire!" from the outside. I infer that language activity from infancy onward has been developed just to meet such situations; hence that during the period of his apparent immobility he was using implicit language processes. Such processes are the only available types of organization which we have any objective right to assume can be used in such a situation[1].

Some unpublished results of experiments by my colleague, Dr Lashley, begin to approach a scientific proof that essentially the same type of responses goes on in implicit thinking as goes on in more explicit types of verbal response. With a delicate apparatus which recorded the tongue movements in two dimensions he was enabled to show that the overt but whispered repetition of a sentence produced a tracing on the smoked drum which was wholly similar except for amplitude to that obtained when he told the subject to think the same thing without making overt movements. He was enabled to verify this again and again. On the other hand if he obtained a standard tracing to a whispered sentence and then gave the subject other work to do and later came back and asked him to think the sentence, there was no obvious correspondence in the two tracings (the original motor set had changed). This is not an argument against our point for I have already shown elsewhere how varied is the musculature of the larynx and the throat. We can write the same word by a dozen different combinations in the holding of the pen. We can speak or think the same word by many different muscular combinations.

I am not afraid, furthermore, of yielding too much to our friendly enemies the introspectionists when I say that the subject himself could observe during the apparent immobile period that he used words and sentences (and that for a part of the time he did not know what he was using!). I am no more afraid to admit this than I am to admit that a person can observe that he himself is laying bricks or playing a piano. I have elsewhere admitted a verbal report method but at the same time I have insisted upon its untrustworthiness for scientific purposes. To know anything worth while for science about my brick-laying I must get a Gilbreth or some other observer to record by motion pictures or otherwise my every act while laying bricks. In other words, scientific conclusions demand instrumentation. I can observe roughly that I have raised a wall four feet high by my day's work, but I cannot determine how many millions of useless movements I have made or how these useless movements could be eliminated by a change in my method of work. Now I hold that the same thing is true of thinking. The subject can observe that he is using words in thinking. But how much word material is used, how much his final formulation is influenced by implicit factors which are not put in words and which he cannot himself observe, cannot be stated by the subject himself.

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[1] In other words, since our assumed explanation is simple and straightforward and adequate to account for all the facts and is in line with what can actually be observed in other activities, the law of parsimony demands that the upholders of 'imagery' and 'imageless thought' should show the need of such 'processes' and demonstrate objectively their presence.

The behaviourist, as well as the psychoanalyst, holds that there are hundreds of such factors involved, some of which require a minute search into the subject's biography as far back as infancy before any adequate answer can be returned. Now two or three years' training in introspection on the observation of thought processes will take our subject no further. It has been abundantly demonstrated, both by the failure of psychologists to get anywhere in the problem of thinking and by psychoanalysts, that such methods simply will not yield results. Such training merely makes him pedantic and insufferably prolix and descriptive of his inward processes. The point I am headed toward here is that if we are ever to learn scientifically any more about the intimate nature of thought other than that which can be obtained by observing the end results—that is, by observing the overt verbally expressed behaviour or the overt ensuing bodily actions—we shall have to resort to instrumentation. The time seems far off when such a thing is possible. While awaiting it the behaviourist has ample with which to occupy himself. Furthermore he is not in such bad straits after all. The physiologists in many cases have to be content with their observations of end results. We know many factors which affect the functioning of the parotid gland. We count the drops of saliva which issue from it under varying conditions of stimulation. We analyse the chemical changes occurring, etc. But what goes on in the gland itself we cannot say. But no one would have the temerity to assume that for this reason there is no physiology of the gland. We can speculate about what goes on inside of the gland, what the function of the unstriped muscular tissue is, why the solution is now thick, now thin, whether the gland would secrete if this or that were done. But those speculations to be of any value must be couched in some kind of terms which will lead not to metaphysical fancies but to some kind of experimental attack. If they do not lead to an experimental attack, no physiologist will long entertain them. I feel that we are in exactly this same position with regard to thinking.

5. FURTHER ELABORATION OF THE PROCESS OF THINKING; SOME OBJECTIONS REVIEWED.

The behaviourist believes that thinking in the narrow sense where new adjustments are made corresponds to the trial-and-error process in manual learning. The process as a whole consists in the organized interplay of laryngeal and related muscular activity used in word responses and substitutive word responses; that is, the motor stage is not always necessarily situated in or even near the larynx. I would write up the process as I infer that it goes on somewhat as follows, drawing my analogy from the wealth of facts we have collected about manual activity. If I hand a subject a mechanical problem box rather large in size and ask him to solve it, I note the movements of the hand, the wrist and even the large muscles of the shoulder as he turns the mechanism from side to side. If, before he finishes solving it, I hand him the same apparatus only reduced to one-tenth of its size he continues his manipulations in approximately the same way, but the amplitude of the muscular response is greatly reduced and many of the movements of the large muscles drop out. The two types of activity are, however, in essence essentially the same. When it comes to thinking we have the following facts: children in large measure think aloud and many adults either think aloud or, if not quite aloud, at least overtly. In others thinking is reduced to such an extent that the bystander can observe only the response of the lips, the jaws and occasionally tongue movements. But the great majority of subjects pass beyond this stage and all observable explicit activity directly connected with the process of thinking disappears (there may still be explicit factors remaining such as walking, wrinkling the brow, sweating, etc.). Having watched in genetic psychology the growth of such processes, having made numerous individuals think aloud in solving their problem, what right have I to assume that the process entirely changes its character when it becomes implicit? Here I call attention to Mr Pear's analysis. He says that the behaviourist catches only the perchings of thought, "When we recall Professor James's description of thought as a series of flights and perchings, it seems that the behaviourist has given us an account of some kinds of perchings, and, fascinating as it is, it reads like a description of flying by an aerodrome mechanic, who sees only the last stages of the aviator's descent." But surely Mr Pear here is hoisted by his own petard. It would be unkind to rob his remarks of their sting by saying that only a well trained aerodrome mechanic can give, after watching a descent, a scientific description of it. The question

I would ask Mr Pear is, what logical right has he to assume that the flight goes on in any different way when it is not under the observation of the mechanic? Surely if we have enough mechanics stationed along the course to watch the entire flight, their combined report would be a faithful account of the flight as a whole. William James's account of transitional states and perchings illustrates very well a fallacy into which Mr Pear and nearly all other psychologists fall, viz., if any part of the process is beyond the range of the bystander's immediate observation he, the bystander, has the right to assume that something unusually interesting and mysterious may go on at the unobserved points. But since the mysterious never happens when the process is under direct observation, the logical fallacy of assuming that something different does go on is obvious. The motive behind James's classical illustration is not difficult to find. It is the motive behind the resistance to the behaviourist's view of thought and its roots lie in mysticism and early religious trends.

Another similar fallacy runs through both Mr Pear's paper and that of Mr Bartlett and Miss Smith, namely that the *expression* of a thought in some kind of implicit or explicit verbal action or in general bodily movement is not necessarily *thought*. Mr Pear uses the illustration of a skater making the figure eight, whereas Mr Bartlett and Miss Smith show dissatisfaction with my simple illustration of a golf player. The figure eight, Mr Pear tells us, is not skating, but is the result of an act of skating. The roots of these objections lie in the fact that these authors are discussing behaviourism not from the behaviourist's own premises but from those of a structural psychologist. Why should a scientific observer of *skating* stop, upon beholding the figure eight made by some particular performer? He might wonder at its regularity, its smoothness and the like, but he would say, "My quest is the goose that laid this golden egg." In studying skating, he would take up the whole system of responses of the skater from and including the moment of fastening on the skates until they were removed. His observation would concern itself with arm and leg movements, the way the ankles function, the compensatory movements of the trunk, with the effort put forth by the skater as shown by the ease and grace of the movements, with the fact as to whether he was perspiring or whether he showed only signs of exhilaration or other emotional changes, etc. Nor would he neglect the tracings made on the ice by the skater's various movements. He would go further and take up the question of the type of training required for such adeptness, of the length of the training period and the age at which such training should begin. In other words, his final data would be sufficient for answering all questions which might be asked about the whole process of fancy figure-skating. After he had made a complete and searching analysis, what would be lacking? The individual's own account, of course. For the sake of completeness we will take it down. Our claim is that, in the great majority of cases, a report by the subject throws very little light upon the act he is engaged in. Let us ask him the question, however, "What were you thinking about while you were skating?" Holt has brought out in his *Freudian Wish* the reply one usually gets to such a question. I shall take the liberty of rephrasing Holt's example so that it will fit the present case. "What was I thinking about? I was wondering whether that 'queen' over there in the red sweater was watching me"!

In a similar vein Mr Bartlett and Miss Smith object to the following statement of mine: "When we study implicitly bodily processes we are studying *thought*; just as when we study the way a golfer stands in addressing his ball and swinging his club we are studying *golf*." Their objection appears in the following words: "But to say that we are studying 'golf' in the second case is to assume that 'golf'—the structure and character of the game itself—is identical with how a given player *plays* golf." I fail to see any special force in this objection. What I want to know when I have a given individual under observation is how *he* thinks or how *he* plays golf. Perhaps I should have worded it differently: When we study an individual's implicit bodily processes we are studying his thinking; and when we study the way a golfer stands in addressing his ball, in swinging his clubs, etc., we are studying the way he plays golf. But let us study many other individuals, both their implicit bodily processes (thought) and their golf playing. Let us write down what we see, and record movements in motion pictures and use all possible methods and instrumentation in our quest. In the end we shall arrive at a monograph on thinking and at another on golf playing. Destroy all books on golf and a man from Mars in a month's time, never having seen the game, could, by watching individuals play, write a decent manual on the rules, structure and *technique* of golf. After having made as searching an analysis as we like upon several players' playing of golf, what will be left out? The individuals' own accounts. Again suppose we take down their overt responses to any questions we may ask and incorporate them into our record. They are of relatively little value. No one since objective studies upon golf have been made trusts the verbal report of a golf player. He will tell you that he never takes his eyes off the ball when making a stroke. The cinema shows that he is a prevaricator. I have never been able to get one valuable scientific statement from a golf player. He does not know how he holds his hands; he cannot tell how he stands, nor the arc he makes with his club, nor whether the arc can vary within wide limits and not affect his stroke. He knows practically nothing about the condition his body is in. To verify this, one needs only to play with a man whose driving has gone off a bit and who has to resort again to trial and error for correction. He asks after every failure, "What did I do that time? Did I bend my body? Did I move my foot?" and so on. A most interesting illustration of the failure of a tennis player to be able to give any worth-while verbal report came to my hands while preparing this paper. *A* took up tennis again after a ten-year period of non-practice. He played against *B*. On the first day his form was pitiful to behold. He stooped at every stroke and twisted his body in every conceivable way. He played five sets and failed to get a game in any set. The score was deuce on only two occasions. On the second day the score was deuce several times and he won one game. He put over several good serves and his form showed great improvement. On the third day there was again steady improvement in form. The returns were swift, and fully fifty per cent. of his first serves were good. On the fourth day he won three games in succession but he was still unable to win a set. All the way through he was terribly discouraged. He had formerly been a fair player with a good serve. He kept saying to his opponent, "I play worse than I did the first day, my wrist is not flexible, I can't get the knack of serving the ball the way I used to, I have forgotten how and when to play the net and to place my balls." It was not until *B* pointed out the objective facts indicated above that *A* was convinced that his playing had improved.

It would be folly to say that in no case is a verbal report wholly without service. To enumerate the places where it is of service is not particularly pertinent to our present discussion.

6. 'CONCEPTUAL' THINKING REALLY A FALLACY.

Mr Bartlett and Miss Smith have, I fear, harkened too much to the logician in their treatment of so-called general relations. They find fault with my simple illustration of building a bridle path. The statement they object to is as follows: "...if the grade is too steep I build my road around the side of the hill." I quote their criticism: "But the real fact of the case is concealed in that statement. In so far as the response is a thought response it is definitely a response to *steepness*; not merely to a particular set of visual reactions, because that would not lead on, of itself, to the further set of muscular and other reactions involved in making the path round the hill; not merely to the steepness of *this* hill, because that also would not take me round it; but especially to *steepness* as a quality common to this and to other situations and independent of any particular context." From the whole history of the way responses grow up I cannot yield this point, and yet it probably would be assented to by most psychologists. Mr Thomson I think has come to my rescue upon this, and I believe he would assent to my further elaboration. One of the first stumbling blocks I had in structural psychology was its treatment of concepts and general ideas. Long before behaviourism took me in tow, I came to the conclusion that such things were mere nonsense; that all of our responses are to definite and particular things. I never saw anyone reacting to tables in general but always to some particular representative. When I began to watch how a child learns to react to words denoting (from the standpoint of logic) a class, the process became clear. When he had his arms full of toys and the stimulus for depositing them was present, his mother would say, "Put them on the table," whether the table was one-legged, an extension table, a library or dining table. The word thus becomes conditioned. The word *table* (any class or abstract word such as animal, justice, mercy, infinity has the same history) becomes thereafter a single individual object, a part of his world of objects, ready to call out a *single definite response* (appropriate to the situation he is in) when he speaks it himself, thinks it or hears it spoken.

In a similar way the definite reaction to the word 'steepness' grows up. The lad takes a walk with his mother over stretches where there are no paths. When he goes up a hill he pants and blows and sweats. His mother says, "Steep, isn't it?" *Steep* becomes substitutable for panting and blowing and sweating. They come to another hill. The mother says "Steep, isn't it? You are tired; let's go round." He learns by trial and error that the word *steep* is followed by sweating, hard work and tired limbs and that this exertion can be avoided by turning to the right or left and circling instead of keeping straight on. When, interested in constructing a bridle path after reaching adult life, he comes to a hill, his whole organization is such that the hill itself (the situation) calls out the word *steep* (conditioned) and steep in turn calls out "turn right or left and circle." I can see nothing in his reactions not explainable by conditioned word responses and simple trial-and-error learning.

As Mr Thomson points out, after reaction to such situations has become habitual, merely being in a situation where he is confronted by a hill leads him to the correct response, namely, circling up its side. Thinking in the sense of implicit word processes need not go on at all. I think, then, we need not agree with Mr Bartlett and Miss Smith when they say, "But what, in this instance, switches me off from the series 'going in this direction' to the series 'going in that,' is the response to a universal quality or relation. That, and that alone, gives us the peculiar characteristic of thinking."

7. 'MEANING' AN EXPERIMENTAL PROBLEM AND NOT A PROBLEM OF PHILOSOPHY OR OF SPECULATIVE PSYCHOLOGY.

This type of argument brings us perilously close to the so-called problem of meaning. I should like to say frankly and without combativeness that I have no sympathy with those psychologists and philosophers who try to introduce a concept of 'meaning' ('values' is another sacred word) into behaviour. At every point we would describe all of psychology in terms of what we see the organism doing. The question of meaning is an abstraction, a rationalisation and a speculation serving no useful scientific purpose. In our seminary at Johns Hopkins University during the past year we went over the various formulations of meaning of the psychologists and philosophers. A more barren wilderness of words it has never been my lot to meet. From the bystander's or behaviourist's point of view the problem never arises. We watch what the animal or human being is doing. He means what he does. It is foolish to ask him while he is acting what he is meaning. His action is the meaning. Hence, exhaust the concept of action and we have exhausted the concept of meaning. It is a waste of effort to raise a problem of meaning apart from actions which can actually be observed. To answer what the church means to men it is necessary to look upon the church as a stimulus and to find out what reactions are called out by this stimulus in a given race, in a given group or in any given individual. Parallel with this query we can carry out another as to why the church calls out such and such responses. This might take us into folk lore and into the influence of the code upon the individual, into the influence of parents upon children, causing the race to project the father and mother into a heavenly state hereafter, finally into the realms of the incest complex, homosexual tendencies, and so on. In other words, it becomes like all others in psychology, a problem for systematic observation and experimentation. I have emphasized these general statements about meaning in this connexion because it is often said that thinking somehow peculiarly reveals meaning. If we look upon thinking as a form of action comparable in all its essential respects to manual action, such speculations concerning meaning in thinking lose their mystery and hence their charm.

8. CONCLUSIONS.

Thinking is then largely a verbal process; occasionally expressive movements substitutable for word movements (gestures, attitudes, etc.) enter in as a part of the general stream of implicit activity. Thinking, in the narrow sense where learning is involved, is a trial-and-error process wholly similar to manual trial and error. Verbal manipulation along one line is checked and stopped and a new line is begun for exactly the same reasons that such processes are checked and begun in manual learning (so-called processes of control[1]). The thinking adjustment is completed when the final word-grouping (sentence or judgment) or overt bodily reaction which comes as the end result of the process of thinking makes the initial stimulus to thinking inoperative or inert; that is, the final reaction, verbal or other, so changes the general state of the organism as a whole that the original stimulating factor can no longer affect the subject. A crude illustration which can properly be carried over to thought is to be found in the hungry hunter's eager search for game. He finds it, captures it, prepares and eats it, lights his pipe and lies down. The hares and quail may peek at him from every corner of the brush, but their driving power for the time is gone.

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[1] Situations *plus* training and organization (the individual's biography) are the only 'control' factors we need in psychology—either for regulating overt bodily action or implicit thought action.

ON LOCAL FATIGUE IN THE AUDITORY SYSTEM.

BY J. C. FLÜGEL.

*(From the Psychological Laboratory, University College,
University of London.)*

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IV. Summary of principal results.

I. INTRODUCTION.

THE phenomena of local fatigue have already been the subject of considerable study from the neurological point of view[1]. Thus it has been shown that some reflexes tire more rapidly than others, that different reflexes may manifest their fatigue in different ways (tremor, phases of strong and weak contraction, slowing and irregularity of rhythm, intermission followed by temporary recovery, etc.), that a reflex in a fatigued state may suddenly give place to an antagonistic reflex as soon as the stimulus is removed or even during its continuance, and that the fatigue is often of an extremely specific character, dependent on the particular area of skin that is stimulated (though not in this case necessarily arising in that area). On the psychological side, too, analogous observations have been made, notably by McDougall[1] in the case of binocular rivalry and in illusions of reversible perspective as manifested in Sinsteden's windmill experiment; while the work of many other investigators, though less specifically devoted to this subject, is not without bearing on it.

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[1] *Vide, e.g.*, Sherrington, *The Integrative Action of the Nervous System*, 214 ff.

Although the study of local fatigue may well appear to be of less obvious and immediate interest than the study of fatigue of a more general nature, it is nevertheless calculated to throw light indirectly on many important problems of work, *e.g.* inhibition and recuperation, and it is perhaps a necessary preliminary to a true understanding of general fatigue (should such a thing exist). Furthermore, when we possess a sufficient number of careful observations on the different kinds of local fatigue both from the physiological and from the psychological sides, a proper classification of the results will probably afford us a considerably increased knowledge of important elementary characteristics of nervous and mental action and of the correlation between them. Further observations on this subject should therefore, it appears, be welcome.

Having myself become familiar with the psychological manifestations of local fatigue through a previous study[2], in which I had repeated and extended McDougall's observations on the windmill, it appeared to me, while assisting in some experiments on the localisation of sound, that the process of binaural localisation was liable to be affected by fatigue in much the same way as illusions of reversible perspective. The experiments in question were undertaken in connexion with the war research then being conducted in the Psychological Department of University College under the direction of Professor Spearman. On the conclusion of this work I was able to take up the problem more systematically than had before been possible, using a portion of the apparatus which had already been devised for the previous researches and which Professor Spearman kindly placed at my disposal for this purpose.

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[1] "The Physiological Factors of the Attention Process," *Mind*, 1906, xv. 329. Especially 340 ff.

[2] "Some Observations on Local Fatigue in Illusions of Reversible Perspective," this *Journal*, 1913, vi. 60.

My conviction that the process of auditory localisation was a suitable one in which to study the manifestations of local fatigue was strengthened by the observations of Urbantschitsch[1], in which the fluctuations in localisation of a tone in the head appeared to behave in a manner not unlike that in the case of the windmill. Being under the impression that such fluctuations in localisation probably depend ultimately upon fluctuations in the intensity of the sensations concerned, I intended to compare such results as I might obtain by the localisation method with those of a second set of experiments directed to studying the effect of fatigue upon the intensity of sounds—a line of research in which Urbantschitsch had again been a pioneer[2], and whose interesting observations on this point had, so far as I knew, not yet been repeated or confirmed. It was at first thought that the two methods might turn out to be merely different ways of measuring the same fatigue phenomena, but as the work proceeded it became abundantly evident that the results obtained under these different conditions were totally independent of one another—fatigue occurring in both cases, but in each case being quite distinct in nature. The results of the two series of experiments may therefore be considered separately. My observers (most of whom took part in both series) had all enjoyed some degree of psychological training —some of them a very high degree. My sincere thanks are due to them for their very willing collaboration.

II. LOCAL FATIGUE AS AFFECTING THE LOCALISATION OF SOUNDS.

1. *Apparatus*. The apparatus used in this part of the work was as follows (see Fig. 1). Into either extremity of a long cardboard tube *A* (containing in the centre a small aperture *Z*) were fitted short rubber tubes *B*, *B′*. At the further end of *B*, *B′* were taps *C*, *C′*, beyond which were further rubber tubes *D*, *D′*, ending in short glass tubes *E*, *E′* which were firmly fastened to the table on which they stood. From *E*, *E′* there extended highly elastic rubber tubes *F*, *F′* which fitted on to the extremities of an apparatus which for the sake of convenience may be called the adjustor. This consisted of two metal tubes *G*, *G′*, soldered together but not intercommunicating, curved at their ends and fastened by means of a hinge *H* to a wooden support *I*, which was screwed on to the table. By means of the hinge the whole adjustor could be freely turned in the horizontal plane through an angle of about 140°. When it was so turned,

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[1] "Ueber subjective Schwankungen der Intensität acustischer Empfindungen," *Pflüger's Archiv*, 1882, XXVII. 436.

[2] "Zur Lehre von der Schallempfindung," *Pflüger's Archiv*, 1881, XXIV. 574.

one of the elastic tubes *F*, *F′* became extended; it being so arranged that when the adjustor was in its 'normal' position, at right angles to the edges of *I* and *A*, the tubes *F*, *F′* were unextended, though one of them became extended at the slightest movement of the adjustor. The total distance between *Z* and the beginning of the metal tubes *G*, *G′* was carefully measured so that when the adjustor was in its 'normal' position the paths *Z–G* and *Z–G′* were of equal length, but as soon as the adjustor was moved to a different position, one path became longer than the other by an extent that bore a definite relationship to the angle through which the adjustor was moved. The precise length of *F* or *F′* for any position of the adjustor could of course easily be calculated by an appropriate trigonometrical process or obtained by direct measurement. In actual practice however this was scarcely necessary, as the adjustor was seldom moved through an angle of more than 30° on either side of the 'normal' position, and the movements were for the most part much less than this, so that the actual elongation of *F* or *F′* during the experiments corresponded very closely to the displacement of the

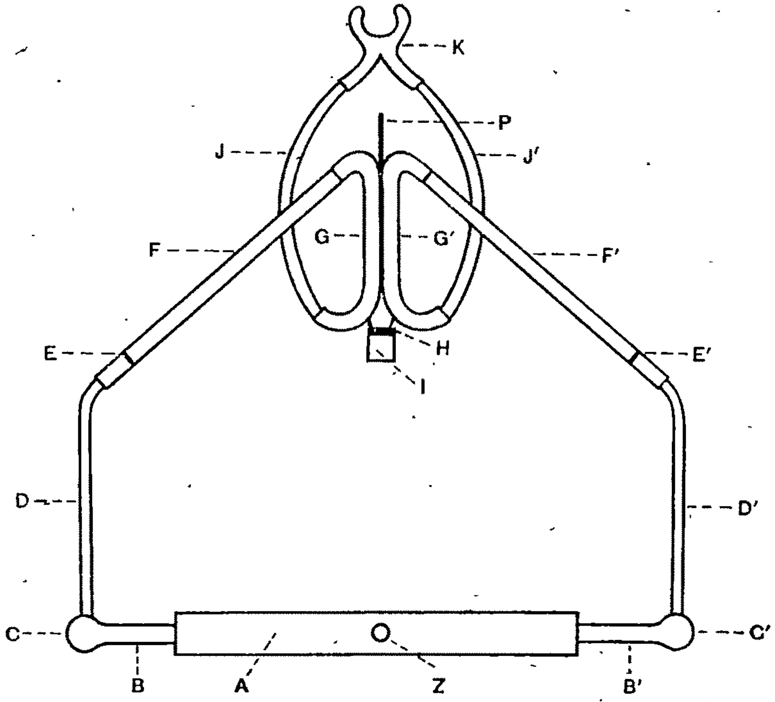

Fig. 1.

adjustor. For this reason, and because of the inevitable roughness of the measurements on the psychological side which made great physical accuracy superfluous, the distance in centimetres through which the adjustor was moved was in every case the measure used in the experiments. To facilitate the taking of the measurements the adjustor was provided with a pointer *P*, immediately beneath which was placed a centimetre scale; the zero of this scale coincided with *P* when the adjustor was in its 'normal' position, so that any departure of the adjustor from the 'normal' could be immediately detected and measured. At the further ends of *G*, *G′* (those nearer *I*) were fitted rubber tubes of equal length *J*, *J′* ending in the head piece of a binaural stethoscope *K*, the ear pieces of which were inserted into the ears of the observer and carefully adjusted in the usual way. The whole apparatus therefore constituted an elaborate double path along which a sound could be conducted from the aperture *Z* to the two ears of the observer; either side of this double path being of such a nature that (by means of an appropriate movement of the adjustor, acting on the elastic tubes *F*, *F′*) it could immediately be made longer than the other side by any desired amount within certain limits.

2. *Procedure.* In a research by Wilson and Myers[1], Rayleigh's statement was confirmed that when a sound travels along two distinct paths to the two ears, it is (other things equal) localised in the middle of the head when the two paths are of equal length, but that if a small difference be made in the length of the paths, the sound is (within certain well-marked limits) localised on that side of the head which corresponds to the shorter path, the apparent displacement of the sound being (again within well-marked limits) roughly proportional to the difference in length of the two paths. This effect has been shown to be connected with differences in the phase of the sound vibration as it reaches the two ears; there being produced an apparent displacement of the sound towards the side on which the vibrations reach the ear in an earlier phase[2]. The problem before us in the present research was to discover whether this function of localisation was liable to be affected by fatigue, and, if so, to what extent.

The procedure in the experiments was as follows. The adaptor having been placed in its 'normal' position, and the observer having placed the ear pieces of the stethoscope in his ears, the experimenter struck a tuning fork and held it while vibrating over the aperture *Z*. The observer then alternately pinched the rubber tubes *J* and *J′* between finger and thumb, the sound being thereby entirely excluded alternately from either meatus. While so doing, he endeavoured to judge whether there was any difference in the intensity of the sound as heard through the two ears. If any difference was detected, one of the taps *C*, *C′* was partly closed on the side which corresponded to the louder sensation and the observations were renewed; this process of adjustment being continued until the observer was satisfied that the sound appeared to be of equal intensity on either side. Thereupon the experiments proper were begun. Each experiment fell into three parts, the general method being the same as that employed by me in my earlier observations on local fatigue in illusions of reversible perspective[1]. There was first a 'preliminary' period, in which the observer having moved the adaptor away from the 'normal' position, endeavoured, with closed eyes, to replace it in such a position that the sound appeared to be localised in the head midway between the two ears. Having brought the adaptor to such a position, he opened his eyes and recorded the position of the pointer on the scale. Several such observations having been thus made by the 'reproduction method' (the initial movement of the adaptor in such observations being to the right and left alternately), the second or 'fatigue-inducing' period was begun. During this the observer left the adaptor at the 'normal' position, and throughout the whole duration of the period heard the sound through one ear only, having placed a metal clip on one of the tubes *J*, *J′*, which entirely prevented the sound from passing through the tube so clipped. At the end of the 'fatigue-inducing' period the observer quickly removed the clip, the sound passed directly to both ears again and the third or 'test' period immediately began. During this the observer made a series of observations by the reproduction method, following exactly the same procedure as he had adopted in the 'preliminary' period.

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[1] "The Influence of Binaural Phase Differences on the Localisation of Sounds," this *Journal*, 1908, II. 363.

[2] We had opportunity to corroborate most of Wilson's and Myers's interesting observations on this point in the course of the previous (war) research at University College already mentioned.

Under these circumstances fatigue, when present, should manifest itself in an alteration in the position of the adaptor required to produce a localisation of the sound in the middle of the head. Thus let us suppose that in the 'preliminary' period when both ears are unfatigued, this position is zero, *i.e.* corresponds to the 'normal' position of the adaptor. If fatigue of the localising function is really present as a result of the stimulation of (say) the observer's right ear[2] during the 'fatigue-inducing' period, this position will, in the 'test' period, no longer give a central localisation of the sound, but a localisation that is displaced from the centre of the head towards the left ear, owing to the preponderant influence of the unfatigued left ear over the fatigued right ear. To bring back a central localisation under these circumstances, the right ear will have to be helped, as it were, by shortening the path along which the sound reaching that ear has to travel, and thus giving it the advantage of the phase difference. The observer will therefore, in this case, have to move the adaptor further to the right, *i.e.* towards the fatigued side, than he did in the 'preliminary' period. The distance through which the adaptor has to be moved for this purpose should afford a measure of the degree of the fatigue, while the time required for the records made by the observer to return to the neighbourhood of those made in the 'preliminary' period should indicate the duration of the fatigue; the successive measurements of the 'test' period giving in this way a fairly accurate picture of the whole course and nature of the fatigue effects produced.

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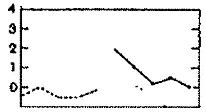
[1] *Op. cit.*

[2] The observer faced the apparatus throughout the experiments.

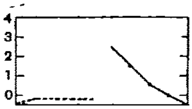
3. *First Results.* Figs. 2–13 record the final results obtained from several observers after a considerable amount of practice (in no case less than three and sometimes as many as ten previous sittings). The dotted lines indicate the scale readings in the preliminary periods, the continuous lines the scale readings in the test periods. The ordinates represent distance in centimetres from the zero point of the scale, *i.e.* the 'normal' position of the adaptor, while each point on the curves corresponds to a scale reading taken at the completion of a judgment made by the observer, so that the abscissae roughly indicate the passage of time (not exactly, since the individual judgments were not necessarily completed at regular intervals). The duration of the fatigue-inducing period in these experiments was in every case three minutes (this period is not represented in the figures). The fork used was $e' = 320$ vibr. per sec.

Inspection of the figures shows at once that the manifestations of fatigue are not uniform. There are marked differences in this respect between one individual and another and in some cases between the two ears of the same individual. The records of observers Co. and Ta. resemble one another pretty closely. Both indicate a moderate degree of fatigue, the two ears being equally affected. Observer Jo. experiences no fatigue with either ear. Observer We. experiences a high degree of fatigue with each ear. Observers Fo. and Fl. show a marked difference in the relative fatigability of the two ears; Fo. experiences no fatigue with the left ear, but very intense and prolonged fatigue with the right ear; Fl.



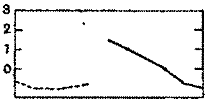
Obs. Co. Right Ear. Duration 2' 25".

Fig. 2.



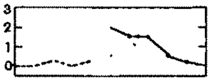
Obs. Co. Left Ear. Duration 2' 18".

Fig. 3.



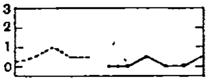
Obs. Ta. Right Ear. Duration 1' 42".

Fig. 4.



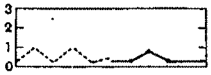
Obs. Ta. Left Ear. Duration 1' 51".

Fig. 5.



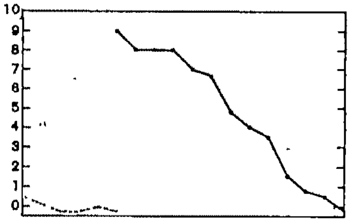
Obs. Jo. Right Ear. Duration 1' 32".

Fig. 6.



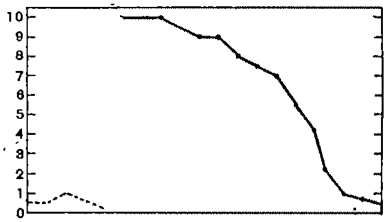
Obs. Jo. Left Ear. Duration 1' 40".

Fig. 7.



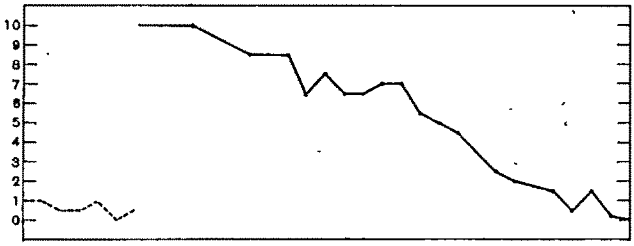
Obs. We. Right Ear. Duration 8' 16".

Fig. 8.



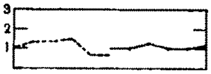
Obs. We. Left Ear. Duration 8' 22".

Fig. 9.



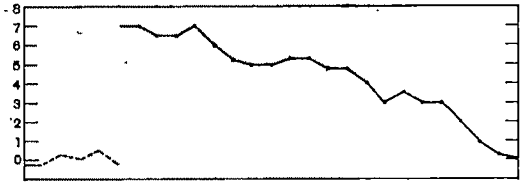
Obs. Fo. Right Ear. Duration 9' 26".

Fig. 10.



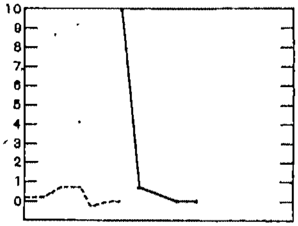
Obs. Fo. Left Ear.
Duration 1' 2".

Fig. 11.



Obs. Fl. Right Ear. Duration 6' 18".

Fig. 12.



Obs. Fl. Left Ear. Duration 30".

Fig. 13.

Figs. 2–13 show displacement in localisation produced in various observers after 3 mins. uniaural stimulation. ------ before fatigue. —— after fatigue.

experiences fatigue of very high intensity but very brief duration with the left ear, and fatigue of more moderate intensity but of long duration with the right ear.

That these curious differences between individuals and between the two ears of the same individual are really characteristic of the behaviour of the individuals or the ears concerned is shown by the close agreement between the results here recorded and those of earlier experiments under the same conditions. With Observers Ta., Fo. and Fl., in particular, quite a large number of experiments have been made, always with the same results; each individual and each ear always reacted in its own characteristic fashion, whenever tested, during a period of over four months. We may therefore conclude that the function of sound localisation under the conditions of the present experiments is liable to fatigue of a kind which displays marked individual and local differences—differences which are, however, constant for a particular individual or for a particular ear over considerable periods of time, showing thus that they are, in all probability, dependent upon more or less permanent characteristics of the psycho-physical processes concerned. The fatigue in question is thus very similar to that manifested in the case of the windmill illusion[1].

Having established this result, further experiments were made with some of the observers, with a view to discovering in what way and to what extent the fatigue would be affected by varying some of the conditions under which the preceding observations had been made.

4. *Influence of duration of the fatigue-producing stimulus.* The first point to be investigated was the effect of varying the *duration* of the fatigue-inducing period. A series of observations was conducted with Ta. and Fl. in which (all other conditions remaining as before) the fatigue-inducing periods lasted ¼ min., ½ min., 1½ mins., 3 mins., 6 mins., or 12 mins., two observations being made with each duration of fatigue-inducing period. It was found that with Ta. the fatigue increased with the increase of the fatigue-inducing period until the latter was of 1½ mins. duration, but that after that increasing duration of the fatigue-inducing periods produced no corresponding effect upon the manifestations of fatigue, which were in every case very similar to those already recorded in Figs. 4 and 5. With the left ear of Fl. the variations in the duration of the fatigue-inducing period produced no appreciable effect whatever; in every case very marked fatigue was observed for a few seconds

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[1] This *Journal*, 1913, VI. 60.

followed by a complete and almost instantaneous recovery[1]. When Fl.'s right ear was fatigued however, a fairly distinct correspondence between the duration of the fatigue-inducing period and the duration of the resulting fatigue became manifest. The second of the two sets of observations for each length of fatigue-inducing period is recorded in Figs. 14–19[2]. It will be seen from these that while the maximum *extent* of disturbance in localisation is reached with a fatigue-inducing period of 1½ mins., the *duration* of this disturbance continues to increase steadily with the duration of the fatigue-inducing period throughout the experiments.

A few observations with fatigue-inducing periods of quite short duration were made also with subject Fo. As in the case of the 3-minute fatigue-inducing periods, no fatigue was manifested in the left ear, but the right ear responded with well-marked fatigue after periods of ½ and ¼ min. The intensity of the fatigue in these circumstances (as measured in the usual way by the movement of the adaptor necessary to produce localisation in the centre of the head) was almost if not quite as great as that shown after the 3-minute periods; but the duration of the fatigue was greatly diminished, readings comparable to those of the preliminary period being now recorded after 20–40 secs., instead of after the lapse of several minutes as in the previous experiments.

From these experiments with fatigue-inducing periods of varying duration it would appear probable that fatigue, where manifested at all, shows in most cases *some* dependence on the duration of the fatigue-inducing period[3]. As this period is prolonged however, there comes a point at which the correspondence between the duration of the fatigue-inducing period and the resultant fatigue ceases to be apparent. The two manifestations of fatigue—*extent* of lateral displacement of the sound and *duration* of this displacement—do not however behave similarly in this respect; the point at which the duration of the fatigue-inducing period ceases to exhibit any correspondence with the extent of the resulting displacement is not necessarily the same as that at which it ceases to correspond with the duration of the displacement. In our present observations with Fl. and Fo. the former correspondence

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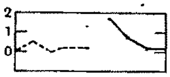
[1] The displacement of the sound was indeed usually so complete that it could not be compensated by any manipulation of the adjustor. It would seem as though the fatigue in this case tended to manifest itself according to the 'all-or-none' principle.

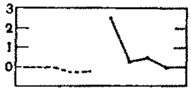
[2] The two sets of observations are in close agreement with one another.

[3] Of the five fatigable ears tested Fl.'s left ear was the only one in which no such dependence was shown in our experiments. Careful observations with very short fatigue-inducing periods would however very possibly have demonstrated the existence of some such dependence in this case also.



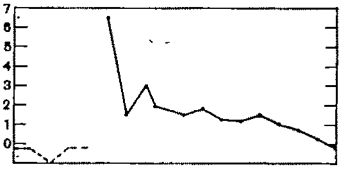
¼ min. fatigue. Duration 37″.

Fig. 14.



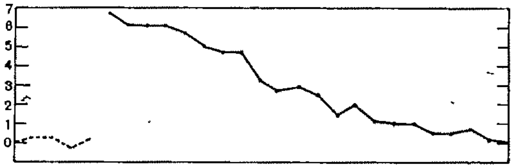
½ min. fatigue. Duration 58″.

Fig. 15.



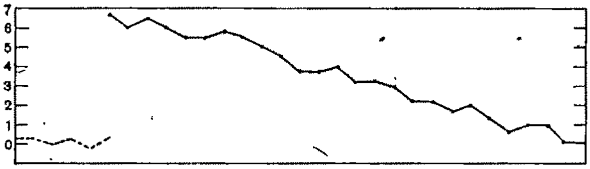
1½ min. fatigue. Duration 3′ 8″.

Fig. 16.



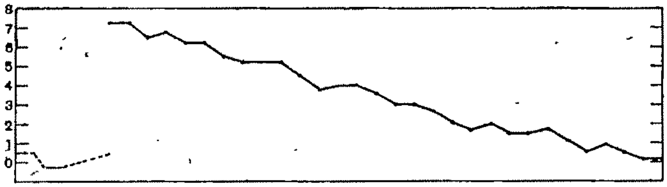
3 min. fatigue. Duration 6′ 9″.

Fig. 17.



6 min. fatigue. Duration 8′ 12″.

Fig. 18.



12 min. fatigue. Duration 12′ 8″.

Fig. 19.

Figs. 14–19 show dependence of fatigue on duration of the fatigue-producing sound. Obs. Fl. Right Ear.

between duration of fatigue-inducing period and extent of displacement shows a tendency to cease at a much earlier point (*i.e.* with a shorter fatigue-inducing period) than does the latter correspondence between duration of fatigue-inducing period and duration of displacement.

To explain these facts it would seem that we must assume the relations between the processes of fatigue and recuperation to vary very greatly in the different perceptive systems involved in our experiments. In some cases (*e.g.* Jo. both ears, Fo. left ear) recuperation can perhaps keep pace with fatigue from the very start, so that no manifestations of fatigue occur (though of course there *may* be subliminal fatigue, and it is possible that even here with a fatigue-inducing period of sufficient length a discrepancy between the two processes would eventually manifest itself in the usual way). In other cases there would seem to be at first an inability of recuperation to keep pace with fatigue, though later on a new equilibrium is established, so that further continuance of the fatiguing stimulus produces no greater preponderance of fatigue over recuperation. This would seem to be indicated by the results with Ta., where increase of the fatigue-inducing periods beyond 1½ mins. produced no corresponding increase of the fatigue manifestations. The establishment of an equilibrium of this kind is doubtless one of frequent occurrence in the nervous system, and is indicated by the results of many experiments on fatigue; very prettily for instance by those of Lehmann with the ergograph[1]. This equilibrium moreover we may perhaps suppose to be of two kinds—sometimes a more or less stable or, as we might say, genuine equilibrium, the prolongation of which shows no after-effects (*e.g.* Ta. both ears); sometimes an equilibrium of presumably a more unstable kind, which is somehow maintained at ever-increasing cost, as shown in the ever-lengthening period required for complete recovery (Fl. right ear, Fo. right ear). In this way we might account for the fact that with the right ear of Fl. there is no increase in the extent of the displacement when the fatigue-inducing period is increased beyond 1½ mins., though there is a steady increase in the duration of the displacement up to 12 mins., and for the fact that with the right ear of Fo. fatigue-inducing periods of ½ sec. or less produce a displacement of almost or quite as great an extent as that produced by 3 minutes' fatigue, though of far less duration.

Finally the relation between fatigue and recuperation may be of such a nature as to produce a partial or entire blocking of a perceptive system after a stimulus of quite brief duration (*e.g.* after fatigue of Fl.'s left ear, when it frequently happened that for a few seconds the sound could not be localised otherwise than on the right, whatsoever the position of the adaptor). In this case no further increase of fatigue occurs with prolongation of the stimulus, and recovery is (at any rate in the case here observed) almost instantaneous[1].

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[1] *Grundzüge der Psychophysiologie*, 62 ff.

A consideration of these effects thus reveals very clearly the peculiar and capricious individuality of the phenomena here under investigation. It would seem that the psycho-physical processes underlying the same conscious states in different individuals or very similar states in the same individual often present marked differences as regards fatigability and power of recuperation. The ultimate nature of these differences —whether structural, functional or trophic—is probably a problem for physiology rather than for psychology and will before long, we may hope, receive a satisfactory explanation at the hands of the neurologists.

5. *Influence of pitch.* The next point to be investigated was the nature of the relation between the fatigue effects and the pitch of the tone heard during the fatigue-inducing period. A number of observations were made with subjects Ta., Fl. and Fo., in which all conditions were precisely the same as in the first set of experiments with 3-minute fatigue-inducing periods, except that forks of 256, 284 and 512 vibrs. were used instead of the fork of 320 vibrs. hitherto employed. In all these observations the results were in every case closely similar to those already obtained for the corresponding ear of the corresponding observer with the 320 fork. We may therefore conclude that, within the limits of pitch difference investigated, the fatigue effects are independent of the pitch of the fatigue-producing sound.

6. *Transference of fatigue.* It remained possible however that while characteristic fatigue effects could be produced for any individual tone,

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[1] It should be noted that in all these cases the fatigue where present does not as a rule manifest itself during the actual continuance of the fatigue-producing stimulus; the effects of fatigue usually become apparent only during the succeeding test period. It is true that on occasion, even during the fatigue-inducing period, the localisation of the sound on the side of the head on which is situated the ear stimulated may become for a few moments less clear and well marked than is usually the case (this was noted especially by Fl. during fatigue of the right ear, not however during fatigue of the left ear, where from the resulting fatigue manifestations such an occurrence would seem perhaps to be particularly likely): but there is nothing at all corresponding to any continuous displacement of the sound towards the centre of the head; at the end of even the longest fatigue-inducing period the sound is often as clearly localised on the stimulated side as at the beginning. In this respect the phenomena here observed are analogous to those of the windmill experiment, where during the fatigue-inducing period (*i.e.* in that case during stereoscopic binocular regard) there is seldom any reversal of the perspective.

these effects might be confined to the perception of the tone in question and would not be transferred to the perception of a tone of different pitch; that, in fact, fatigue might be specific to the processes underlying the perception of a particular tone and not common to the whole unilateral auditory apparatus. In order to study this point, a number of experiments were made in which one fork was used in the preliminary and test periods and another fork of different pitch in the fatigue-inducing periods. Subjects Ta., Fl. and Fo. again took part in these experiments. With Ta. the following combinations of forks were tried (the fork used in the fatigue-inducing period being stated first, that used in the preliminary and test periods second): 256–320, 256–512, 384–320; with Fl. 256–320, 256–384, 256–512, 512–384, 384–320; with Fo. 384–256, 512–320. With Fl. some additional experiments were also made with one of the forks damped by riders so as to produce a more dissonant interval. In every case the results coincided with those obtained in the earlier experiments, each individual peculiarity being exhibited in the same way and to approximately the same extent as when the same fork was used throughout. It thus appears that the fatigue here under consideration is not confined to the processes underlying the perception of a particular tone, but is common to all perception of tone in the ear stimulated.

7. *Influence of loudness.* A further question to be investigated was the possible influence of the loudness of the tone upon the fatigue effect. For this purpose fork 320 was used to produce a tone of three different grades of intensity. For the medium intensity the conditions were precisely the same as in the earlier experiments. For the weak intensity the aperture Z was closed by a layer of cotton wool of sufficient thickness and density to make the sound when heard through the stethoscope but slightly above the threshold for continuous audibility. For the strong intensity an appropriate resonance box was suspended above the aperture, the tone being thereby greatly reinforced and appearing as loud as could pleasantly be borne, when heard through the stethoscope. Ta., Fl. and Fo. took part in these experiments, two observations with each intensity being made with Ta. and Fl. and one with Fo. Apart from the variation in loudness, all conditions were as in the first series of experiments. Here again it was found, contrary to the expectation of the observers, that the varying conditions made no apparent difference whatsoever to the fatigue effects, which were manifested to approximately the same extent as usual (and with all their individual peculiarities) for all three grades of intensity. We may therefore conclude that—

within wide limits at any rate—the fatigue effects are independent of the intensity of the fatiguing stimulus.

8. *Influence of consciousness.* In all these experiments the observers had listened with close attention to the tone during the fatigue-inducing period so that the auditory sensation itself was by far the most constant and prominent thing in consciousness during that period. To discover what would happen when the tone ceased to enjoy this prominence in consciousness was the object of a further series of experiments undertaken with subjects Ta. and Fl. The subjects were given (*a*) a passage from a book to read silently and attentively, (*b*) a series of pictures of an exciting character to be attentively contemplated, (*c*) a series of additions in one of Kräpelin's *Rechenhefte* to be performed as rapidly as possible. None of these distractions during the fatigue-inducing period produced any significant effect, the fatigue being manifested just as when full attention had been given to the fatiguing stimulus.

A further step was therefore taken, and it was decided to exclude the fatiguing sound from consciousness altogether. This was done by calling in the assistance of a second experimenter, who was provided with two small drum sticks with which he beat a rapid and continuous series of loud blows upon a tin box throughout the fatigue-inducing period; the noise thus created being sufficient to make the sound of the fork completely inaudible to the observer. Observers Fl. and Fo. took part in these experiments, and with both it was found that the characteristic fatigue effects manifested themselves as on previous occasions, in spite of the inaudibility of the fatigue-producing stimulus. With Fl. an additional series of control experiments was performed, in which a large screen was placed between the observer and the experimenter, and an assistant experimenter manipulated the taps *C*, *C′*, in such a way as to exclude the sound from one of the paths when so desired—thus obviating the necessity for the use of clips (adjusted by the observer), as employed during the fatigue-inducing period in the other experiments. In the absence of any auditory clue the observer was now unable to tell when the stimulus began, which ear was being stimulated or whether there was any stimulation at all. Under these conditions there were made two experiments with stimulation of the right ear, two experiments with stimulation of the left ear and two experiments without any sounding of the fork at all, the observer being in each case ignorant of the conditions of the experiment. On each occasion the appropriate response was made; stimulation of the right or left ears produced all the usual characteristics of fatigue following such stimulation (moderate and prolonged fatigue in the case of the right ear, intense fatigue of very short duration with the left ear), while absence of stimulus was followed by records in the test period that were extremely similar to those made in the preceding preliminary period. These results therefore afford additional evidence of the genuineness of the phenomena observed, and at the same time show that the fatigue resulting from inaudible stimulation is uncomplicated by the rather intense binaural stimulation occasioned by the loud beating on the tin box. We may therefore conclude that the conscious appreciation of the sound is not a necessary condition for the production of the fatigue here under consideration.

9. *Does uniaural fatigue influence binaural hearing?* The problem towards the solution of which the next series of experiments was directed was concerned with the influence of fatigue upon localisation during continuous binaural hearing. In the case of those observers whose fatigability is different in quality or quantity according as the sound is led to one ear or the other, it would seem not improbable that with continuous binaural hearing there might occur illusory alterations in the apparent locality of a tone, due to the differential action of fatigue upon the two perceptive systems on whose co-operation the power of localisation depends—alterations to which observers with more equal fatigability of the two systems concerned might perhaps be less exposed. The question is of considerable practical interest, since the process of binaural localisation of a continuously heard sound is one that was called into play in several military operations during the war, it being therefore a matter of importance to select the most sensitive and reliable observers for this purpose. With a view to studying this point, our observers listened continuously to the sound of the 320 fork for 15 minutes, the sound being led to both ears and the adaptor being placed at the zero point on the scale. Every 30 seconds the observer moved the adaptor rapidly away from this point (to right and left alternately) and then, bringing the adaptor in again towards zero, recorded a judgment as in the previous experiments. Such an experiment was performed twice with Ta., Fl. and Fo. and once with Co. and Jo. It was thought that if asymmetrical fatigability of the two ears were in any way disadvantageous to the process of localisation during continuous binaural hearing, the records of Fl. and Fo. in this experiment would be inferior to those of the other observers.

The results showed no such influence at work. The mean variation of the 30 readings taken during the experiment worked out as follows (using the second experiment, where two were performed): Co. ·55 cm., Ta. ·19 cm., Jo. ·85 cm., Fl. ·58 cm., Fo. ·60 cm. While there was evidence of individual differences in these figures (Ta. exhibited a distinctly smaller variation than do any of the other observers), there was no indication of any distinct superiority of Co. and Jo. over Fl. and Fo. Nor again was there any significant difference of this kind as regards the general shape of the curve representing the course of the successive judgments. None of the curves exhibited any steady tendency for the apparent zero point, as recorded in the judgments, to move continuously to the right or left. It might have been supposed that with Fo. the increasing fatigue of the right ear system, acting in combination with the unfatigability of the left ear system, would have manifested itself during continuous binaural hearing in a tendency to make the apparent zero point move progressively to the left. Similarly with Fl., it might have been expected that the difference in the nature of the fatigability shown by the right and left ear systems would have influenced in some way the curve representing the successive judgments—though the precise nature of the effects upon the curve are less easy to foretell. The absence of any demonstrable effects due to these causes upon the curves for the two observers in question and the close similarity of their curves to those of the other observers seem to indicate fairly clearly that asymmetrical fatigability, such as that exhibited by Fl. and Fo., does not appreciably influence the localisation of tones during continuous binaural hearing. The reason for the non-appearance of any manifestations of fatigue under these circumstances—whether, for instance, it is to be found in the fact that the fatigue only takes place during uniaural hearing, or whether the fatigue is present but is prevented from manifesting itself—is a matter of considerable theoretical interest and one on which some light might perhaps be thrown by further experimentation: it was not possible however to enter into the matter at the time when the experiments here recorded were being carried out.

10. *Relation between localisation and intensity.* As mentioned in an earlier part of this paper, it was originally supposed by the author that the changes in the localisation of a tone produced by fatigue would very possibly prove to be largely or wholly reducible to changes in the relative intensity of the sensations originating from the two ears. Before proceeding to study these latter changes in the second part of the research, it was decided to make a few observations with a view to ascertaining whether intensity differences, in any way corresponding to the localisation differences, could be easily observed with the present apparatus and experimental procedure. As a preliminary step in this direction an

attempt was made to compare the threshold for movements of the adaptor obtained (*a*) by judgments of localisation in the usual way, (*b*) by judgments of intensity differences between the sensations coming from the two ears. This was done in the following manner. For (*a*) the adaptor, instead of being manipulated by the observer as hitherto, was controlled by the experimenter, who placed it in turn over the various points of the scale in the irregular manner followed in the method of right and wrong cases. The observer meanwhile was blindfolded so that he was unable to see the movements or position of the adaptor, and with his two hands he tightly pinched the two tubes of the stethoscope, so that the sound coming through the tubes was normally inaudible. On receiving a signal from the experimenter, he removed his hands from the tubes for a few seconds, carefully listened to the sound and expressed a judgment as to its rightness or leftness. This done, he again pinched the tubes and waited for the next signal; several judgments being thus made for each position of the adaptor. In (*b*) the procedure was the same, except that, on hearing the signal, the observer, instead of opening both tubes, opened first one tube and then the other in rapid alternation, listening alternately to the sounds from the two sides and comparing their intensity; ultimately making a judgment as to which of the sounds was the louder. In both (*a*) and (*b*), to ensure uniformity of sound, an electrically driven tuning fork of 250 vibrs. was substituted for the forks hitherto used.

Observations were made in this way with Fl. and Fo. It was found with both observers that the threshold for localisation (*a*) was far lower than that for intensity (*b*). While in the case of (*a*) errors were very seldom made when the adjustor pointed to more than 1·5 cm. on either side of the zero mark (the actual thresholds, as calculated, were ·49 cm. with Fl. and ·73 cm. with Fo.), in the case of (*b*) errors were made freely at all positions of the adaptor, no satisfactory threshold being obtainable, since 'right' judgments occurred even at the extreme left of the scale and *vice versâ*. This demonstration of the comparative insensibility to intensity differences (which agrees with the recent work of Stewart and Hovda[1]) indicates that, under the conditions of our experiments, the (much finer) process of localisation cannot depend to any considerable extent upon intensity differences coming from the two ears—or at any rate upon a *conscious appreciation* of these differences. It would therefore appear that the factors which underlie the localisation process must probably be looked for elsewhere than in *mere* awareness of differences of intensity—a discussion of the possible nature of these factors being however beyond the scope of the present paper.

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[1] "The Intensity Factor in Binaural Localisation," *Psychol. Rev.* xxv. 1918, 242.

A more important result for our immediate purpose is to be found in the consideration that, owing to the great difference in delicacy between the perception of localisation differences and the perception of intensity differences, the former must almost certainly involve different psycho-physical elements from those concerned in the latter. In other words, our own above recorded experiments on fatigue of localisation can scarcely be concerned with the same processes as those which figured in Urbantschitsch's already-quoted experiments on auditory fatigue as measured in terms of intensity difference. The two processes concerned being thus distinct, a comparison of their behaviour under the influence of fatigue becomes of all the greater interest. With a view to this comparison, we turn now to a consideration of the experiments undertaken to repeat and extend the observations made by Urbantschitsch.

III. LOCAL FATIGUE AS AFFECTING THE INTENSITY OF SOUNDS.

1. *Methods*. Urbantschitsch's demonstration of the influence of fatigue on the intensity of auditory sensations was made as follows[1]. Two rubber tubes led to the two ears of the observer, one tube being partially closed if necessary, so that the tone of a tuning-fork, when held opposite the distant ends, first of one tube, then of the other, produced sensations of equal loudness. This preliminary adjustment having been made, the tuning-fork was struck loudly and held steadily opposite the open end of one of the tubes until it ceased to be audible to the observer. It was then quickly moved to a new position opposite the end of the other tube, whereupon the tone would become audible again and remain perceptible for a brief period before its final disappearance. As an alternative procedure the fork was, after its first disappearance on the fatigued side, moved rapidly to and fro from the end of one tube to the end of the other; when this was done, periods of silence (corresponding to the moments when the sound was led to the fatigued ear) alternated with periods of audibility (corresponding to the moments when the sound was led to the unfatigued ear) until the final disappearance of the tone.

In the present experiments, which followed closely those of Urbantschitsch, these two procedures were designated methods *A* and *B* respectively. Further procedures in studying the same phenomena were adopted as follows.

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[1] "Zur Lehre von der Schallempfindung," *Pflüger's Archiv*, 1881, XXIV. 574.

In method *C*, as soon as the observer reported the first disappearance of the tone, the fork was suddenly damped, moved away from the end of the tube and then gently struck again either with a small and soft mop or with a fleshy part of the finger, and, after a period of four seconds from the cessation of the sound, moved to and fro before the ends of the two tubes as in method *B*, until the observer reported that the sound had again become inaudible.

In method *D* the vibrations of the fork during the fatigue-inducing period (as we may here call the period elapsing between the original sounding of the fork and the first disappearance of the tone) were not allowed to die down as in the previous methods, but were prevented from falling below a certain (fairly high) degree of amplitude by the fork being struck anew at short intervals (usually five seconds). At the end of the fatigue-inducing period (the duration of which could now be controlled by the experimenter) the fork was damped and then struck again (and allowed to die down) as in method *C*.

Methods *C* and *D* have the advantage that two forks can be employed if desired—the fork used for testing the existence of fatigue in the second part of the experiment (test period) being not necessarily the same as that employed for producing fatigue in the first part of the experiment (fatigue-inducing period). These methods were therefore considered especially suitable for studying the question of the transfer of fatigue from the perception of one tone to that of another. On the other hand, for reasons that will appear in a few moments, these methods were not without difficulties of their own. There was therefore added—

Method *E*, in which the period taken for the vibrations of a fork to become inaudible to the observer (when the fork was struck with a given degree of intensity) was, first of all, carefully noted in a number of preliminary observations. In the experiments proper the fork used for testing, on being struck, was held away from the end of the tubes during the fatigue-inducing period; meanwhile another fork (of the same or of different vibration rate, as required) was held opposite the end of one of the tubes to produce fatigue. On the elapse of a period rather shorter (usually seven seconds less) than the average of the disappearance-times recorded in the preliminary observations, the fork to which the observer had hitherto been listening (the fatigue-producing fork) was suddenly removed and the other (test) fork was—without any appreciable intervening interval—moved to and fro as in the previous methods.

In the case of all these methods, the actual experiments were always preceded by a number of preliminary observations in which the fork used was moved to and fro before the ends of the tubes, the observer meanwhile comparing the intensity of the sensations coming from the two ears and ultimately noting whether the sensations disappeared together or, if not, which sensation lasted longer. If (as was generally the case) there was noted any inequality as regards intensity or duration, a small amount of cotton wool was placed in the mouth of the tube corresponding to the louder side and the process was repeated until the observer was satisfied that the sensations were of equal intensity and duration.

Experiments with a fork of 320 vibrs. were made by methods *A*, *B*, *C* and *D* upon a number of observers (notably Ta., Jo., Fl. and Fo. who had taken part in the previous experiments on localisation and upon a new observer In.). Fatigue was shown in the case of all these observers and by all four methods; but not all the methods were found equally suitable or convenient for demonstrating the existence of the fatigue: each method has to some extent its own peculiar advantages and disadvantages.

Method *A* is perhaps the easiest and most satisfactory of all from the observer's point of view; fatigue being, as a rule, easily demonstrable in this way even to a totally unpractised subject. On the other hand, the value of the results obtained is apt to be very dependent on the accuracy of the preliminary adjustments made to obtain sensations of equal intensity in either ear. Thus if the fatigue effect is more marked after fatigue of the right ear than after fatigue of the left, it is often uncertain whether this is due to a genuinely greater fatigability of the right ear or to the fact that the sound as conveyed to the left ear is—quite apart from fatigue—somewhat louder than that conveyed to the right. Nor can this difficulty be altogether avoided by any degree of care in the preliminary adjustments for obtaining equality of loudness; for it soon became apparent that the relative intensity of the sensation from the two ears is subject to frequent slight variation due to causes other than fatigue. The cause and nature of these variations are matters of some interest on their own account (to some extent the variations appear to be connected with pressure changes in the middle ear, for it was often observed that they were affected by swallowing movements on the part of the observer), but they were beyond the scope of the present investigation, in which their influence had merely to be eliminated as a disturbing factor. In method *A* such elimination can only be accomplished by frequently ascertaining afresh whether the sounds from the two ears were of equal intensity or not. Such repetition of the initial testing had necessarily to be undertaken from time to time with the other methods also, but in these methods there was usually some indication from the actual experiment itself as to whether one of the variations in question had taken place. While comparing the sounds from the two ears when the fork was being moved to and fro (as was done in all methods except *A*), the observer would often notice immediately if the customary difference (due to fatigue) between the two sounds was either lacking or unusually intense, and, his suspicions being thus aroused, he would demand a retesting. The indications for retesting being thus given as a rule in the actual course of the experiment, the retesting process had seldom to be resorted to except when really needed; considerable time and trouble being thereby saved.

While method *B* is in this respect superior to method *A*, it has in common with *A* the drawback that one fork only can be used for both fatiguing and testing. The interval of four seconds between the first disappearance and the reappearance of the sound in method *C* was introduced in order to allow of the experimenter changing the forks where necessary, in order to study the transfer of fatigue from one tone to another. This method was however also used with a single fork throughout, for the purpose of obtaining data exactly comparable to those obtained with two forks.

Methods *A*, *B* and *C* all suffer from the disadvantage that the duration of the fatigue-producing stimulus is determined by the period required for the gradually diminishing vibrations to reach the limit of audibility—a period that differs according to the nature of the fork used, the strength with which the fork is struck and the acuity of the observer's hearing. This indeterminateness of the length of the fatigue-inducing period (though subsequently found to be of little importance) is on theoretical grounds obviously undesirable in an experimental procedure. Method *D* was therefore resorted to with a view to overcoming this disadvantage. In this method the duration of the fatigue-inducing period was under the control of the experimenter; except where otherwise stated, this period was forty seconds. Method *D* has the further advantage that the intensity of the fatigue-producing stimulus is kept more nearly constant during the fatigue-inducing period, instead of being allowed to decline continuously throughout that period, as in *A*, *B* and *C*.

The introduction of the four seconds interval in methods *C* and *D*, though it had the advantage of allowing two forks to be used if desired, was found to bring in a serious fresh drawback not present in methods *A* and *B*: it made the observation of the fatigue effects a good deal more difficult and uncertain. It was for this reason that method *E* was devised; by which it was hoped to combine the advantage accruing from absence of interval, as in methods *A* and *B*, with that of allowing the use of two forks, as in methods *C* and *D*. Method *E*, however, has a disadvantage of its own, in that considerable preliminary experimentation is required, and was for this reason only used in experiments bearing on the question of the transference of fatigue.

After this somewhat elaborate discussion of the methods used, it will be possible to deal quite shortly with the results obtained—the more so, since these results presented for the most part a striking uniformity and regularity; the difficulties of the experiment lying, it would seem, rather in the conditions of observation and in the liability to disturbance by irrelevant factors than in the nature of the fatigue phenomena themselves.

2. ***First results.*** As already mentioned, fatigue effects were obtained for all the observers and for every method used, though by some methods more certainly and clearly than by others, for reasons that we have now explained. Table I gives the number of experiments (out of a total of eighty for each observer, forty for each ear) in which more or less clear manifestations of fatigue were obtained by the various methods.

Table I. *Showing number of observations (out of a total of twenty for each ear) in which fatigue was manifested by different methods.*

	Right Ear.				Left Ear.			
Method	*A*	*B*	*C*	*D*	*A*	*B*	*C*	*D*
Ta.	10	9	7	6	10	10	7	7
Jo.	9	8	10	7	9	10	8	8
Fl.	10	8	7	8	10	9	7	7
Fo.	8	9	8	6	10	9	6	6
In.	9	9	7	7	10	8	8	8

It will be seen at once that the observers present no such clearly marked individuality as they did in the experiments on localisation; nor is there found any significant difference between the two ears of the same observer, such as was apparent in some cases there. Fatigue is here manifested by every observer and in about the same number of individual experiments. Furthermore, a consideration of the introspections offered by the observers suggests very strongly that, had it not been for the interference of irrelevant factors and the difficulties of observation, fatigue would have manifested itself in every one of the experiments.

Nor was there anything in these experiments to indicate the existence of any considerable difference between the observers as regards the intensity or the duration of the fatigue produced. As regards intensity, the number of times that a tone was heard at the non-fatigued ear after it had ceased to be audible at the fatigued ear was about the same for all the subjects. (The actual number of times varied from 1 to 8 in successful experiments, the average being, for Ta. 3·3, for Jo. 3·4, for Fl. 3·1, for Fo. 2·7 and for In. 2·8.) As regards duration, the greater difficulty that all observers experienced with methods *C* and *D* as compared with methods *A* and *B* (corresponding to the smaller figures recorded for the former methods in Table I), when examined in the light of the observers' introspective comments, appears to be due to the fact that the most marked period of the fatigue effect had already passed during the four seconds interval that occurred in methods *C* and *D*. If this is so, it would seem probable that with all the observers the fatigue effect is of short duration only and declines rapidly in intensity from the start.

3. *Influence of duration of the fatigue-producing stimulus.* With a view to obtaining more precise information on this question of the duration of the fatigue, a second series of experiments was undertaken with observers Ta., Jo., Fl., Fo. and In., in which method *D* was used as before, with the modification however that, whenever the first test gave evidence of fatigue (*i.e.* whenever the sound was heard longer at the unfatigued ear than at the fatigued ear), a second test was immediately carried out in the same way as the first; if this second test gave evidence of fatigue, a third test was immediately undertaken, and so on indefinitely until the sound was heard to die away as nearly as possible simultaneously at both ears. In this way it was hoped to obtain a rough measure of the duration of the fatigue effect. The whole series of experiments was performed twice, at first with a fatigue-inducing period of forty seconds, as in the earlier experiments, then with a fatigue-inducing period of three minutes; the aim being to obtain evidence as to the nature of the dependence (if any) of the duration of the fatigue effect upon the duration of the preceding fatigue-inducing period. Each observer made twenty experiments with each length of fatigue-inducing period (ten with each ear).

The figures in Table II indicate how frequently a given number of tests had to be made before equality between the two ears was reached. Thus the figures in the first column show how often fatigue manifestations failed to be forthcoming even in the first test, those in the second column show how often fatigue was manifested in the first test but not in the second, those in the third column show how often fatigue was manifested in the first and second tests but not in the third, while those in the fourth column show how often fatigue was manifested in three or more successive tests.

Table II. *Showing number of tests made before re-establishment of equality between the two ears.*

	Fatigue-inducing period of 40 secs.				Fatigue-inducing period of 3 mins.			
	No fatigue in 1st test	Fatigue in 1st test	Fatigue in 2nd test	Fatigue in 3rd or later test	No fatigue in 1st test	Fatigue in 1st test	Fatigue in 2nd test	Fatigue in 3rd or later test
Ta.	3	14	2	1	4	12	4	0
Jo.	2	17	1	0	2	14	3	1
Fl.	4	12	2	2	3	16	1	0
Fo.	5	15	0	0	3	14	2	1
In.	4	12	3	1	4	15	0	1
Total	18	70	8	4	16	71	10	3

Two points emerge with some clearness from these figures. In the first place, it is evident that there is no appreciable increase in the duration of the fatigue manifestations when the fatigue-inducing period is increased from forty seconds to three minutes; with all the observers the fatigue has apparently attained its maximum in the former period.

Secondly, it is clear that the fatigue is, in nearly all cases, of quite short duration, being for the most part manifested only in the first test. The actual duration of a test varied of course slightly from one experiment to another (being dependent both on the observer's acuity of hearing and on the strength with which the fork was struck), but in the great majority of cases was between ten and fifteen seconds and very seldom above twenty seconds. Allowing therefore a total period of twenty seconds (to include both the individual test period and the interval of four seconds between the fatigue-inducing period and the test period, or between two successive test periods), we may say that in general the fatigue effect is longer than twenty seconds (because fatigue is usually manifested in the first test) and shorter than forty seconds (because fatigue is usually not manifested in the second test). Introspective remarks by the observers indicate that the former figure is probably nearer than the second: it was fairly often reported that at the beginning of the first test the difference in intensity between the two sounds (and therefore presumably the fatigue effect on which this difference depends) was a good deal more marked than at the end, whereas equality of the sounds is usually reported as being observable from the very beginning of the second test. This evidence in favour of the rapid decline and disappearance of the fatigue effect is in harmony with the conclusions that may be drawn from a comparison of the results obtained by methods *A* and *B* with those obtained by methods *C* and *D* (see above). The greater difficulty in observing the fatigue effect by the latter methods would seem to be due to the fact that the most marked manifestations of fatigue no longer occur after a few seconds' interval. On the other hand, the few observations recorded in Table II, in which a fatigue effect distinctly longer than usual was observed, are in several cases open to suspicion as being due to the more permanent variations between the relative acuity of the two ears to which we have already drawn attention. Readjustment of the relative loudness of the sounds (in the manner already described) was found necessary after eight of the twenty-five observations reported in the last two columns on either side of Table II, while only fifteen such readjustments had to be made following the 141 observations in which fatigue was manifested in one test only.

Summing up all this evidence, we may therefore conclude with some degree of assurance that the fatigue here in question is seldom of more than thirty seconds' duration and that it usually lasts for rather less than this period.

4. *Influence of loudness.* The next question to be investigated was concerned with the relation of the fatigue effect to the intensity of the fatigue-producing tone. Some light on this point is already thrown by a comparison of the results obtained by methods *C* and *D* respectively (Table I). In method *C* the sound was allowed to decline steadily in intensity throughout the fatigue-inducing period until the limit of audibility was reached; in method *D* the tone was kept throughout at fairly high intensity. The results however show no considerable difference in the frequency with which fatigue was observed by the two methods. A further series of forty experiments bearing on this point was undertaken by observer Fl. Method *D* was used in these experiments, but in half the total number of cases the fork was struck quite softly with the finger, while in the other half it was struck with the hammer in the usual way but with considerably greater force. The results showed no appreciable difference as regards the fatigue produced by the soft and loud stimuli respectively, being in both cases very similar to those already recorded in Table I. We may conclude therefore that, within wide limits at any rate, the fatigue is independent of the intensity of the fatiguing stimulus.

5. *Influence of pitch.* The influence of pitch was next examined.

Experiments for this purpose (by method *B*) were made by observers Ta., Fl. and Fo., ten observations being made by each observer with each of the following forks: 256, 384, 512. Again no significant differences were forthcoming, the results being in every case very similar to those recorded for fork 320 in Table I. It appears therefore that, within the limits tested, the fatigue is independent of the pitch of the fatiguing stimulus.

6. *Transference of fatigue.* The next question that arises is that connected with the transfer of the fatigue from the perception of a tone of one pitch to the perception of a tone of another pitch. Urbantschitsch had already made some interesting observations on this point and had concluded that no such transference occurs. Our own methods *C* and *D* were originally devised with this end in view, but the difficulties in observing the fatigue effect by these methods were found to be even greater when two tones were employed than when, as in the experiments hitherto described, a single tone was used throughout. This increased difficulty appeared to be connected with the 'shock' of surprise experienced by the observer on suddenly hearing a tone different in pitch from that to which he had been listening during the fatigue-inducing period; this shock made judgment difficult at the beginning of the test period, precious seconds being thereby lost. Method *E* was therefore devised to avoid the loss of time necessitated by the interval in methods *C* and *D*. The results—which had before been rather disappointing, the observers recording their judgments with some degree of hesitation—immediately improved, and a series of experiments was carried through by observers Ta., Fl. and In. and a new observer Pe., which—so far as can be judged from introspective evidence—was quite satisfactory and reliable.

This series of experiments includes in all 214 separate judgments with all combinations of forks 256, 320, 384 and 512. Of these judgments only 22 (or rather more than 10 per cent.) showed any distinct tendency for the sound to be heard more loudly on the unfatigued than on the fatigued side. That these latter judgments are to be attributed to errors of observation or to the more permanent variations in loudness to which we have already more than once referred rather than to the influence of fatigue, is shown by the fact that there were nineteen judgments in which the *opposite* effect occurred—the sound being heard longer on the fatigued side than on the unfatigued side. We may therefore conclude with very little hesitation that there is no transfer of fatigue from the perception of a tone of one pitch to the perception of a tone of another pitch—our observations on this point being thus in agreement with those of Urbantschitsch. In this respect the fatigue here under consideration differs markedly from the localisation fatigue previously studied.

A question of some interest that arises in connexion with this result is concerned with the degree of difference in pitch that is sufficient to interfere with transfer. Experiments bearing on this question were conducted with observers Fl. and Pe. Two forks of 256 vibrs. were used, one fork being provided with movable riders, so that the pitch could be varied to any desired extent within certain limits. The experiments were conducted according to method *E* and the procedure of right and wrong cases was followed as regards the sequence of the various degrees of difference in pitch between the two forks. It was found that the results were similar to those usually obtained in the attempt to measure a threshold, in that there existed no sudden point of transition, the judgments indicating presence or absence of transfer being on the contrary freely interspersed over a certain area. This area was however a fairly narrow one, a relatively small difference in pitch being sufficient as a rule to prevent transfer. The actual threshold calculated[1] from all the judgments was, in the case of Fl. ·52 vibrs. per second (with a mean variation of ·17), in the case of Pe. 1·46 vibrs. per second (with a mean variation of ·38).

This result is possibly not without some importance for the theory of hearing, since, if we may argue from the absence of fatigue transfer from one process to another that the processes concerned are distinct, it would show that separate and distinct psycho-physical processes are called into play in the perception of two tones as soon as their pitch differs by something in the neighbourhood of 1 vibr. per second. It is interesting to note too that the threshold for the transfer of fatigue is in the neighbourhood of the threshold for the perception of pitch difference. Indeed in the case of the present two observers the correspondence turned out to be extraordinarily close; measurements for the threshold of pitch discrimination undertaken immediately on the conclusion of the fatigue experiments giving thresholds of ·49 (m.v. ·19) in the case of Fl. and 1·38 (m.v. ·36) in the case of Pe. Nothing of course can be safely inferred from observations on two subjects only, but, taken in connexion with the general correspondence in the nature and neighbourhood of the two thresholds, these figures would suggest that the psycho-physical processes concerned in the fatigue are the same as those which correspond to the actual conscious perception of a tone, and are therefore central and not peripheral in situation.

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[1] In accordance with the method described by Spearman, "The Method of Right and Wrong Cases without Gauss's Formulae," this *Journal*, 1908, II. 227.

7. *Influence of consciousness.* Some further light is thrown on this last point by our final series of experiments, in which an endeavour was made to study the effect on the fatigue manifestations produced by excluding the fatigue-producing sound from consciousness. As in the case of the experiments on localisation, exclusion from consciousness was attempted in two ways: first, by the distraction caused by carrying on some mental task; secondly, by drowning the sound by a louder noise. The experiments were in this case carried out with observers Jo., Fl. and In., method *D* being employed throughout.

The distracting mental tasks (for the most part mental multiplication) were found to exercise little influence on the fatigue, it being almost impossible to exclude the sound from consciousness for the whole period, and the tone being usually, as a matter of fact, discoverable by introspection as persisting in the background of consciousness during the greater part of the fatigue-inducing period.

Much more marked however was the effect of the complete exclusion of the sound from consciousness by the noise of beating on the tin box. Out of twenty observations for each person Jo. reported only two observations which could be interpreted as giving indications of fatigue, Fl. one such observation and In. four such observations. When compared with the similar figures for the same method in Table I, it at once becomes apparent that exclusion from consciousness prevents, or at least very seriously diminishes, the liability to fatigue. The dependence of the fatigue on conscious perception (which is, again, in marked contrast to the behaviour of the localisation fatigue studied in the previous section) agrees with the conclusion that may be drawn from the results of the threshold experiments as to the central situation of the psycho-physical processes concerned in this fatigue.

IV. SUMMARY OF PRINCIPAL RESULTS.

The process of binaural localisation of a tone is frequently subject to fatigue when one ear has been previously stimulated. The fatigue manifests itself in an apparent displacement of the tone towards the unfatigued side. Its effects can be compensated by causing the sound affecting the fatigued side to arrive at an earlier phase than the sound affecting the unfatigued side. During the process of recuperation the apparent displacement of the sound diminishes—as a rule gradually—and finally disappears.

This fatigue is manifested to a different extent in different individuals, and sometimes even in the two ears of the same individual.

This fatigue exhibits usually some dependence on the duration of the fatigue-producing stimulus. There would appear to be a point however (varying with different individuals) beyond which continuation of the fatigue-producing stimulus causes no corresponding increase of the fatigue manifestations.

The fatigue is largely—perhaps quite—independent of the pitch and intensity of the fatigue-producing stimulus.

The fatigue produced by a tone of one pitch is transferred to the perception of a tone of another pitch.

The fatigue is unaffected by distraction, and occurs even when the fatigue-producing stimulus is rendered inaudible by a simultaneous louder sound affecting both ears.

This fatigue does not produce any appreciable effect upon localisation during continuous binaural hearing, even in the case of individuals who are more fatigable on one side than on the other side.

Uniaural stimulation produces a second kind of fatigue, which manifests itself in diminished intensity of the auditory sensation produced by stimulation of the fatigued ear, so that after uniaural stimulation a tone of continuously diminishing intensity will become inaudible on the fatigued side at an earlier moment than on the unfatigued side, though before fatigue it had become inaudible on both sides at the same moment.

This fatigue is of brief duration (seldom if ever more than thirty seconds) and would seem to decline rapidly as soon as the fatiguing stimulus is removed.

This fatigue is experienced by all observers to approximately the same extent.

It is largely independent of the duration, pitch or loudness of the fatiguing tone.

Fatigue produced by a tone of one pitch does not affect the perception of a tone of different pitch, except within limits that correspond roughly to the limits of pitch discrimination.

No manifestations of this fatigue are observed when the fatiguing tone is rendered inaudible by a louder stimulus affecting both ears.

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A STATISTICAL SURVEY OF ARITHMETICAL ABILITY[1].

BY DANIEL J. COLLAR.

I. *The problem.*
II. *School marks for research purposes.*
III. *Description of the tests and the method of marking.*
IV. *Results of the survey.*
V. *Summary of conclusions.*

I. THE PROBLEM.

IN common educational practice, it has been customary to regard arithmetic as a single subject, and therefore to assume that a child who excels in any part of it will excel in the whole. The lad who does well, for example, in problematic work has been expected to be quick and accurate in simple computation. The assumption is not an unreasonable one, for arithmetic is a deductive science. Its most complicated applications depend ultimately on its simplest principles, and its practical value disappears when computation is inaccurate. The unity of arithmetic is implied, rather than explicitly asserted, in various psychological researches, such as those of Starch[2] and Carey[3], who have each furnished correlations between it and other subjects of the school curriculum.

While the inter-dependence of the various forms of the subject cannot be ignored, it is manifest that arithmetic has at least two aspects, widely differing in psychological character: (1) the mechanical, as shown in computative skill, (2) the intelligent, as shown in the ability to reason arithmetically. The former appears to have little relation to intelligence, in the ordinary acceptation of this term. It should depend on the acquisition and use of memory habit. The latter invokes the power to manipulate the unforeseen, to formulate hypotheses, to test these critically, and to carry out self-devised plans to their logical conclusions.

Howell[4] declares that arithmetical ability is a 'myth,' *i.e.* there does not exist, as a rule, in the individual child an ability that may be expected to function in all forms of arithmetic.

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[1] A condensation of a thesis approved for the degree of Master of Arts (Education) London University, 1919, and of a paper read before the British Psychological Society (Education Section), March 9th, 1920.

[2] Starch, *J. Educ. Psychol.*, 1912, III. 415 ff.

[3] Carey, this *Journal*, VIII. 170–182.

[4] Howell, *Foundational Study of the Pedagogy of Arithmetic*, 147.

This claim that arithmetical ability is a 'myth' appears ultimately to rest on the pioneer experiments of Stone and Courtis, both of America, who investigated this problem. The former[1] examined very large numbers of children, and found that classes who did well in computation were often inferior in arithmetical reasoning, and *vice versa*. But his methods were such as to test the relative efficiency of the teaching in different schools rather than to discover individual tendencies. Courtis[2] had for his main object the establishment of norms of performance in the various branches of arithmetic. His work in that direction is outside the scope of these remarks. His experimental conditions, however, scarcely warrant his general conclusions about arithmetical ability. He explicitly states that skill in one branch of arithmetic gives little or no indication of skill in another. "If skill in addition does not influence skill in subtraction," he says, "it would seem that there could be little hope of transfer in any other subject also." And again, "It seems as if each operation and part of an operation is learned as a separate and unrelated activity." He has even gone so far as to specify fourteen kinds of arithmetical ability. This view of arithmetic, the writer, adopting Spearman's[3] terminology in another connexion, has been tempted to call 'non-focal.' It is difficult to accept this atomistic explanation. Put in such a way, it suggests a denial of intellective unity in the individual mind—a position some have shown a tendency to adopt since formal training became a subject of acute controversy.

Winch[4] and Burt[5] have, on the other hand, independently found high correlations between computation (rules) and problems. Burt has suggested that there is a special form of ability, operative in arithmetical work generally. The question is one of importance. For the practical teacher, it is convenient to know to what degree ability in any form of arithmetic has prognostic value. For the educational psychologist, the determination of the existence of mental relationships becomes a useful contribution towards a science of education. There thus seemed ample justification for a further inquiry into the whole matter. The writer had the opportunity of doing this in a London elementary school.

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[1] Stone. (*a*) *Arithmetical Abilities*; (*b*) *J. Educ. Psychol.*, 1910, I. 608 ff.

[2] Courtis. (*a*) *J. Educ. Psychol.*, 1910, I. 610; (*b*) *Elementary School Teacher*, Chicago, X. 58–74, 177–199; XI. 171–185, 360–370, 528–539; XII. 127–137.

[3] Spearman and Hart, this *Journal*, V. 53.

[4] Winch, *J. Educ. Psychol.*, 1910, I. 557–589.

[5] Burt, *Educational Abilities*, 58.

II. SCHOOL MARKS FOR RESEARCH PURPOSES.

For the purpose of the research, which took the 'statical' form of a survey, school marks, though available, were not utilised for reasons which may be briefly summarised, and which, it is believed, are of general validity.

(1) For accurate scientific measurement, they are usually far too crude, and often lie within too narrow a range. Teachers have too little time and opportunity for a careful and detailed estimation of the value of school work.

(2) School marks are given to a great extent for disciplinary reasons, and thus represent factors other than mental ability. The writer knows one successful teacher, who prefers to allot marks in composition solely for the effort to improve, and thus does not take into account the absolute value of the exercise.

(3) School marks frequently contain spurious elements, as, for instance, marks for neatness in working problems in arithmetic, and for spelling in composition exercises.

(4) When a series of marks reaching over a considerable period is used, conditions are improved but little, for dislocation is caused by absences; and averaging is not satisfactory, for exercises vary in difficulty.

(5) When, as in composition, a definite objective standard for marking is impracticable, it is very difficult for the marker to preserve a uniform standard while dealing with a large number of exercises.

(6) In the present investigation, school marks did not cover the whole field of inquiry.

Starch's[1] examination of school marking sufficiently reveals its unreliable character for a psychological investigation. In general, the value of the marking for this purpose tends to be discounted by the fact that it is done for the benefit of the pupil. The main object is the detection of errors with a view to the prevention of their recurrence.

III. DESCRIPTION OF THE TESTS AND THE METHOD OF MARKING.

The tests which form the main topic of this article were carried out between Easter and Whitsuntide, 1918, in a boys' school of south-east London. The five classes that took the tests corresponded with Standards 3, 4, 5, 6, 7. As the educational year was just beginning, the children were examined in the work of the standards they had just left. The complete results for about 200 children form the basis of the statistical work done.

The number of tests given was large, and the period over which they extended was 6 weeks. When a child through absences had omitted several tests, his work was excluded from consideration in the final results. To secure uniformity of conditions, and to eliminate, as far as possible, features undesirable in a careful inquiry, all the tests were personally conducted by the writer. As it was necessary for obvious reasons to limit the period of investigation, the exercises could not always be set under uniformly favourable conditions, and therefore the usual effects of practice in producing improvement were not always evident. It was felt essential that the boys should not do the work in a fatigued or listless state. Therefore, all the testing was done in the morning, or early afternoon. In order that the children should be stimulated to do their best, the whole series of exercises was made to become, from their point of view, a school competition for success in which certain small prizes were given. The exercises were first marked as ordinary school work, the results being well advertised to the pupils. Afterwards, the papers were re-marked for the purpose of this investigation. On the whole, therefore, the exercises may be accepted as representing the genuine efforts of the pupils.

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[1] Starch, *Educational Measurements*, Chap. II.

The assumption directing the research was that arithmetic has a trifold aspect:

(*a*) Computation, the mechanical aspect.
(*b*) Rules, the knowledge aspect.
(*c*) Problems, the intelligence aspect.

Each of these was tested five times for each class, and mental or oral arithmetic as a typical class exercise was also included.

(*a*) For computation, each pupil on each occasion was provided with a sheet of paper, containing a number of cyclostyled exercises in simple addition, subtraction, multiplication, and short division. The number of examples of each of these four 'fundamental' processes was so great that no child could finish any one of them in the allotted time of 3 minutes. The tests were so planned that all the possible combinations of figures were included. The order in which the four 'fundamentals' appeared varied from paper to paper. On the first paper for each class, the order was addition, subtraction, multiplication, division; on the second, subtraction, multiplication, division, addition; and so on. The sums in any one fundamental were of uniform length for each class. Each class had its own standard of length of sum for each rule. Below are given typical specimens of the tests in subtraction.

Standard 7	Standard 6	Standard 5	Standard 4	Standard 3
8234759	700814	85463	9307	581
4456382	463572	26048	4546	232

The procedure for each test in computation was uniform. The general directions were, "Work carefully, and try to get as many examples right as possible. Don't revise your work." Before the working of the next test on the paper, an interval of two minutes was allowed, no boy being permitted to look at his next task. During this interval, a little pleasant conversation between the writer and the boys usually took place. In this way, the four fundamentals were tested once each in 20 minutes. Five such sessions completed the computation work for one class.

The exercises were marked separately for speed and accuracy. Each complete sum involves a certain number of operations or steps. In a simple addition sum, for example, the number of steps is usually one fewer than the number of digits. The total number of steps, which the child worked in 3 minutes, was taken as his measure of speed. It was recognised that multiplication has addition steps, and that division has subtraction steps. In subtraction, care was taken to insert a due proportion of steps where 'borrowing' was not required. Errors would have been far fewer, if steps requiring 'borrowing' had been invariably inserted, for children tend to make this process habitual, instead of performing the necessary elementary act of reasoning.

It is legitimate to take, as the measure of computative *efficiency*, the number of correct steps, or operations done in a certain time. But a score thus made depends primarily on the speed of working. On this system, a child who completes 100 steps in computation, and makes five errors scores 95, one who completes 94 steps, all correct, scores 94. Clearly this plan will not do for the measurement of accuracy. In the present tests, accuracy was measured by the frequency with which errors occurred. Thus if a child worked 175 steps, and made five errors, his score for speed was 175 and for accuracy 35, *i.e.* 175 ÷ 5. Infinity thus became a possible score, but as correlations were worked out by Spearman's Rank formula, no difficulty was experienced.

(*b*) In all classes, except the lowest, five tests each were set in rules, the knowledge aspect of arithmetic. These included most of the types of sums that had been taught the children at some time in their school career. There was no rigid time limit. The children were directed to work the examples carefully, and to give up their exercises as soon as they had finished.

Each test included six examples, and five was the maximum mark obtainable for a sum. The assumption was thus made that all the examples were equal in difficulty, as all represented subject matter that had at some time or other been taught the children. Marks were deducted for defective knowledge of method, or of the general facts of arithmetic. A child lost marks, for example, who could not obtain the correct remainder to a sum in short division by factors, or who assigned the wrong number of yards to a mile. In accordance with the plan to consider in marking only the characteristics peculiar to the phase of arithmetic being tested, no notice was taken of errors in calculation.

(*c*) Five sets of arithmetical problems were given to each class. Each set contained six examples, varying considerably in difficulty. As the object was not to obtain norms of performance applicable to the school as a whole, and probably approximating to general validity, each class was provided with its own special work, and again no time limit was imposed.

A more elaborate system of marking than that adopted for rules was deemed necessary. The more difficult problems should receive the more marks. But it is impossible to decide beforehand upon the relative difficulty of the examples. It is not satisfactory to value problems according to the number of steps involved, for the difficulty of a problem depends on many conditions other than this. Stone[1] set his problems to classes outside the main scope of his experiments, allowed unlimited time, and held that the order of difficulty of the examples corresponded in each case with the number of correct solutions. In the present case, as the schools were working under war conditions, it was considered undesirable to impose burdens on other teachers, but it was decided to determine the relative difficulty of the problems from the results of the actual testing, and to allot marks accordingly. The broad principle was adopted that the assessment value of a problem should depend in some inverse way upon the number of children who worked it correctly[2]. The plan of a sliding scale was eventually chosen, and formed the basis for marking. Table I shows the sliding scale for Standard 4 (number of children, 41).

Table I.

No. of correct solutions to problem	Maximum mark
35—41	4
30—34	6
25—29	8
20—24	10
15—19	12
10—14	14
5— 9	16
1— 4	18

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[1] Stone, *Arithmetical Abilities*, 13.

[2] See also Buckingham, *J. Educ. Psychol.*, 1914, v. 204.

Thus, it was possible to recognise that the partial solution of a difficult problem may be more deserving of credit than the complete solution of an easy one. The marking of correct steps towards a solution had to be done in an arbitrary way, but was based on the principle that, in an arithmetical problem, the whole is greater than the sum of its parts. While partial solutions in proportion to their value received credit, the noetic act of thought, as manifested in the comprehension of the whole situation, was considered as the vital factor. Every endeavour was made to mark merely for arithmetical reasoning. Inaccuracy of computation was disregarded, the time element was eliminated, and if a child's knowledge of facts proved defective, for example, if he stated that 2240 yards make a mile, he was not penalised.

Five tests were given to each class in mental or oral arithmetic. The examples included computation and rule-work. They were dictated at a suitable speed, and the children wrote down the answers. One mark was allowed for each correct answer.

It has been stated that one object of the investigation was to determine whether a specific mental factor operates in all arithmetical work, or whether the non-focal views, previously outlined, represent a closer approximation to truth. In order that this point might be dealt with, some means had to be devised for the elimination in the final comparisons of any factor operative in school work generally, such as general intelligence, diligence, and long-distance memory. Thus, in addition to the arithmetic, tests were set in composition, spelling, reading, geography, and history.

Among the subjects for composition were—how we depend on one another; what children can do to help in war time; advantages and disadvantages in war time of Britain's insular position. No maximum mark was fixed. All ideas that contributed to the proper development of the theme were valued, due credit being given for any points that appeared to be of special merit. The sum of such marks was found, and duly accredited for subject matter. For literary style, these marks formed the basis, deductions being made for grammatical faults and other blunders of style for which the child might reasonably be accounted responsible. Thus, a child who scored 30 marks for matter, and who was penalised for 12 blunders, scored 18 for style, his total being 30 + 18, *i.e.* 48. Fertility and originality of thought were taken as the most important factors in composition. It is a task of no little difficulty to assess composition exercises with any degree of confidence, and the adoption of a quasi-objective standard seemed an experiment worthy of trial. It is by no means impervious to criticism, but it had at the time a distinct suitability to the school where it was tried.

For spelling, two sets each of 60 words were dictated. The words ranged from easy to difficult, and the number of correctly spelt words was taken as the score.

Reading was examined as an ordinary school subject. It was tested orally, marks being given for fluency, accuracy and expression. No use was made of the scales[1] for the testing of reading, which have been framed by Thorndike, Gray, Starch and Courtis.

The amount of knowledge of the facts of history or geography which the boys had acquired was measured by two sets of twenty questions each in each subject. The two lowest classes took history only. The questions, generally answered in a word or two, tested memorising power, and not the ability to make deductions from geographical or historical data. Two was the maximum mark for an answer, and 40 for a whole paper.

Furthermore, it appeared advisable to obtain measures of general intelligence, as independent checks to the work. To every class but one, six tests of intelligence were given, each repeated twice. In Standard 7, for certain reasons, there could be but four. All the intelligence tests were of a linguistic character. Simpson[2], describing his researches into the correlations of school subjects, mentions four tests which, he claims, bear a high relation to general intelligence. These are the mutilated text or completion test as devised by Ebbinghaus, opposites, the memory of unrelated words, and the memory of passages of literature. Examples of these four tests were each given twice in the early stages of the testing. The results, however, were not in all cases satisfactory. Some of the tests require very careful adjustment before they can be given with advantage. Slight modifications of procedure may make all the difference between failure and success.

The tests in opposites were not carried out with any relation to reaction times. The children were required to write out the opposites to a number of words presented to them. Though the two tests showed adequate correspondence with each other, the results would have been more reliable if larger lists of words had been presented.

Neither of the memory tests proved particularly satisfactory, and both were abandoned. There is no general agreement about the connexion between memory and intelligence. While Ebbinghaus[1] has denied that there is high relationship between the two, Burt[2] in a series of tests of immediate memory found a close connexion, but, in common with other investigators[3], hesitated to draw very definite conclusions.

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[1] (*a*) Starch, *Educational Measurements*; (*b*) Chapman and Rush, *Scientific Measurement of Class-room Products*.

[2] Simpson, *Correlations of School Subjects*, 8, 9, 10.

Results of greater reliability were obtained in further intelligence tests, including analogies or mixed relations, the re-arrangement of disordered sentences, and a modified form of the completion test.

IV. RESULTS OF THE SURVEY.

For the purposes of the survey, the method of correlation was employed throughout, and the first crude results were treated by means of Spearman's Foot-Rule formula[4]. The R-values obtained from this formula were changed into the r-values of the Bravais-Pearson formula by means of conversion tables, calculated by Spearman from the empirical formula $r = \sin\left(\frac{\pi}{2} . R\right)$. As the number of raw correlations calculated in the survey was over 2000, it will readily be seen that the simpler method, though less exact, was the more suitable for use. The probable error of R may be taken as $0{\cdot}43 \div \sqrt{N}$, where N = number of children tested, and it is thus constant for any one class. As the numbers of children in Standards 7, 6, 5, 4, 3 were 36, 25, 43, 41, 51, the probable errors of R may be taken in order as ·07, ·086, ·065, ·067, ·06. All of these quantities must be raised proportionately when R-values are converted into r-values, and again when the correction formula is applied. To furnish proof of real correspondence between two series, correlation coefficients should be considerably in excess of their probable errors. Generally speaking, correlations in the survey below ·30 are to be viewed with some lack of confidence.

To make due allowance for the child's variability in performance, use was made in the investigation of the Spearman correction formula[5],

$$r(S, T) = \frac{r_1(S, T)}{\sqrt{r(SS_1) . r(TT_1)}}.$$

Here $r(S, T)$ stands for the true correlation between S and T, $r_1(S, T)$, the average of the raw or single correlations between S and T, $r(SS_1)$ and $r(TT_1)$, the average reliability coefficients of the raw gradings of

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[1] Whipple's *Manual*, 386.

[2] Burt, this *Journal*, 1909, III. 143.

[3] (*a*) Winch, *Ibid.*, 1904–5, I. 134; (*b*) Whipple, *op. cit.*, 386.

[4] Spearman, this *Journal*, 1906, II. 89 ff.

[5] Udny Yule, *Introduction to Theory of Statistics*, 213.

S and *T*. This formula[1] is based on the assumption that the magnitude of errors of measurement between two mental characters can be estimated by comparison with the size of discrepancies found when measuring repeatedly each of the characters in turn. $r_1 (S, T)$ should theoretically be the geometric mean of the raw correlations[2]. But the difference between this and the arithmetic mean is in practice very small, and accordingly, the examples of Spearman and Burt[3] have been followed, the arithmetic mean being selected for ease of calculation. The correction formula has been used freely, and indeed in some cases where reliability coefficients have been scarcely high enough, and the conditions of its legitimate employment barely realised.

It was not thought expedient to pool into one statistical table the results for several groups of children[4]. There are distinct objections to the compilation of the marks obtained from a number of children at all stages of mental development. As a result of the separate treatment of each class in the survey, certain differences between older and younger children are indicated. One of the points that emerges in this discussion is that each class has its own peculiar characteristics, and the presentation of the results of each separately should thus be a powerful check to premature generalisation. On the other hand, a very strong case will be advanced if it can be shown that certain tendencies exhibit themselves in every group, whatever its distinctive features may be.

A typical specimen of the inter-correlations between the results of the tests in the speed of working one of the fundamentals is shown in Table II.

Table II.

A, B, C, D, E = the five tests in speed of multiplication, Standard 7.

	A	B	C	D	E
A	—	·85	·59	·67	·65
B	·85	—	·51	·67	·69
C	·59	·51	—	·51	·55
D	·67	·67	·51	—	·61
E	·65	·69	·55	·61	—

Average, ·63.

An examination of this table shows that, even in exercises as simple and uniform in character as these, the individual child exhibits marked variability in performance. The average reliability coefficients, shown in Table III, are not always high, the lowest being ·56 in the case of multiplication for Standard 6. In a few cases in various parts of the school, single correlations between two measurements of the same fundamental are lower than ·50, though in no single case is there one below ·40.

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[1] Spearman, this *Journal*, 1910, III. 271.

[2] Brown, *Essentials of Mental Measurement*, 83.

[3] Burt, this *Journal*, 1909, III. 110.

[4] See Spearman, *Ibid.*, 1910, III. 278.

Table III.

Average reliability coefficients in the four fundamentals.

	Standard				
	7	6	5	4	3
Addition ...	·62	·68	·68	·67	·64
Subtraction ...	·77	·63	·67	·67	·70
Multiplication ...	·63	·56	·78	·76	·73
Division ...	·72	·63	·75	·70	·62

The correlations generally appear lower than those found in many other investigations. This is due to the comparative homogeneity of the groups tested. These, coinciding with the Standards, were far more uniform in attainments than groups formed on age-bases would usually be. The latter will in general cover a far greater range of ability than a class group, and both the clever and the dull children, despite the factor of variability in performance, will exercise a steadying effect on correlations. In proportion as such influence is removed by effective organization, so will emphasis be thrown on the performance of children of moderate and fairly even ability from the point of view of the intellectual class level, and the natural lack of steadiness in the child's work will produce the greater disturbance.

The average corrected coefficient of correlation between the four fundamentals (see Table VIII) is ·74, Standard 6 having the lowest ·67, and Standard 7 the highest ·78. Standard 6, a remarkably homogeneous group, shows a corrected correlation of only ·35 between subtraction and division. Apart from this, the lowest corrected correlation is ·55 between addition and division in Standard 3.

Table IV.

Percentages of error in the fundamentals.

	Standard				
	7	6	5	4	3
Addition ...	1·4	1·6	2·1	2·5	2·8
Subtraction ...	3·6	3·6	5·0	9·9	8·3
Multiplication ...	2·1	1·7	2·1	3·8	8·1
Division ...	1·5	1·9	1·9	8·5	18·6

The computation exercises were marked for accuracy as well as speed. It will be remembered that the measure of accuracy adopted was the frequency with which errors occurred, or the ratio of error to the work done. Table IV summarises the results considered from this standpoint.

In the progress through the school, as the processes become familiar to the children, the frequency of errors diminishes, and finally, viewed in relation to the work done, the proportion of error may be insignificant. In the top class, almost 98 per cent. of the steps worked are quite correct. Similar results are found in nearly all the computation exercises. This is the main justification for adopting a speed standard only, as a measure of computative ability. It matters little, in most cases, whether the number of steps or the number of *correct* steps is taken as the measure. The former criterion is less satisfactory where in the lower classes the frequency of error becomes high.

Although the proportion of error appears to be so small, the effect has been to make a quarter of the complete sums wrong. The fact that small slips have such great results is one of the most powerful of arguments for making the elementary operations of computation as mechanical and certain as possible. Arithmetical teaching can hardly be accounted a failure when the child gets 98 per cent. of his calculations right. On the other hand, the young clerk who gets a quarter of his results wrong is not a very valuable asset to the business world. The point has little psychological value, but it has an important bearing on the question of the alleged inaccuracy of school children in computation.

Generally speaking, so small is the frequency of error, that the effect of a single slip on the child's rank for accuracy may be considerable. A cursory examination of the original mark sheets would show clearly that any single exercise of an average child, marked for accuracy, is of small value as a guide to other performances. Table V gives a typical specimen of inter-correlations for accuracy.

Table V.

Multiplication (accuracy), Standard 7.

	A	B	C	D	E
A	—	·31	·45	·14	·43
B	·31	—	·25	·14	·17
C	·45	·25	—	0	·29
D	·14	·14	0	—	·14
E	·43	·17	·29	·14	—

Average, ·23.

They are, as a rule, very low, and in many cases approach zero. Even a few negative coefficients have been obtained, and in few instances are the correlations sufficiently in excess of their probable errors to be of significance. That is to say, while a few children may be consistently accurate, and a few consistently inaccurate, it is extremely difficult to grade a group as a whole for accuracy, or to decide, in the general case, whether one boy is more or less exact in calculation than another. In a competitive examination, when the whole future of the child may lie in the balance, it thus appears inexpedient and unjust to penalise him with too great severity for slips in calculation, the tendency to which is in all probability enhanced by the abnormal conditions of the occasion.

This statement is not intended to undervalue the importance of accuracy in calculation. For here work must be perfect or it is useless. Perfection is not a necessary attribute to proficiency in the subjects of the curriculum in general.

So low were the reliability coefficients, shown in Table VI, for accuracy in computation, that the conditions for the legitimate application of the Spearman correction formula were not realised. The final measure of accuracy taken for each child was simply the ratio of the number of steps in computation done in all the twenty exercises to the total number of errors. By the use of this measure, it was possible to make a comparison between accuracy in arithmetic and proficiency in other subjects. The former shows little sign of being a significant mental factor in the intellectual ability of young children.

Table VI.

Reliability coefficients for accuracy in the fundamentals.

	Standard				
	7	6	5	4	3
Addition ...	·20	·16	·40	·16	·40
Subtraction ...	·29	·19	·35	·50	·43
Multiplication ...	·23	·05	·30	·32	·34
Division ...	·10	·22	·18	·32	·45

The relation of speed to accuracy has been the subject of previous experiments. W. Brown[1] found, in three groups of children tested, positive correlations of ·13 ± ·08, ·24 ± ·10, ·33 ± ·10. These correlations are small in relation to their probable errors. The general inference is that speed and accuracy tend to go together. Thorndike[2] has arrived at a similar conclusion.

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[1] Brown, *Essentials of Mental Measurement*, 103, 104, 145, 146.

[2] Thorndike, *J. Educ. Psychol.*, 1914, v. 537.

Table VII.

Correlations between speed and accuracy in computation.
(Uncorrected correlations of the amalgamated totals.)

	Standard				
	7	6	5	4	3
Addition ...	·13	·40	·23	·03	·32
Subtraction ...	−·13	−·01	·07	·38	·35
Multiplication ...	−·06	·13	·08	·13	·64
Division ...	·50	·14	·28	·43	·52

The results of the survey, appearing in Table VII, are somewhat less decisive than the previous ones. In seventeen cases out of twenty, positive correlations exist, supporting on the whole the conclusions of Brown and Thorndike. In so far as speed and accuracy are both saturated with general educational ability, or special arithmetical ability, the quicker will tend to be the more accurate. But there may sometimes occur in a class group, a proportion of the slow-and-sure type large enough to produce a negative correlation. Under the conditions of the experiment, the child was aiming at both speed and accuracy at once. On account of the instruction not to revise his work, the child made more mistakes than would be viewed with complacency in ordinary class exercises. This suited the purpose of the experiment. The children worked at their maximum speed, and betrayed something like their maximum tendency to inaccuracy.

It is interesting to note that the amount of work done by any one class in any one rule varied but little from test to test. Where the greatest slackening in the rate of performance in a class took place, it was usually found due to some unfavourable circumstance immediately preceding the testing. Such conditions too will explain at least partially the reason why there was not found a steady increase in efficiency with the progress of the tests. Again, there was but little variation in the percentage of errors made by any one class in any one fundamental. There thus appears justification for reliance on the results of a single test, when the object is to ascertain the standard of proficiency attained by a class as a unit in both the characters of speed and accuracy.

Generally speaking, the corrected correlations between the fundamentals, appearing in Table VIII, are much too high to support the non-focal view of arithmetical ability. No claim is advanced that the ideal of mental measurement has been reached in the present investigation. More refined methods for the detection of inter-relationships of mental capacities have been suggested[1]. The general effect of such is to increase correlations[2]. Apart from experiment, it seems hardly to be expected that ability in one of the fundamentals should be widely different from ability in another, for in all there are powerful common elements—the mechanical manipulation of figures, the inter-connexions of the arithmetical tables, and the inclusion of the time factor.

Table VIII.

Corrected correlations between the fundamentals.

Standard	Add.-Sub.	Add.-Mult.	Add.-Div.	Sub.-Mult.	Sub.-Div.	Mult.-Div.
7	·85	·81	·64	·90	·69	·80
6	·85	·96	·58	·65	·35	·61
5	·88	·75	·60	·79	·69	·66
4	·80	·81	·71	·77	·73	·78
3	·83	·74	·55	·94	·78	·65

A specimen of the inter-correlations between the separate tests in problematic arithmetic appears in Table IX. The causes that militate against high correlations include the variability in form of the individual child, due to a complexity of conditions. His existing physical state will be more or less favourable to mental testing. Then, assuming that the child's mental store is adequate to the occasion, the recall of the appropriate principle depends on suggestion[3]. The mental act required for the solution of a problem is essentially noetic. The form of the situation must be cognised as a whole. In so far as the child achieves proficiency

Table IX.

Inter-correlations for Arithmetic (Standard 7).

Tests A, B, C, D, E in each case.

	Problems						Rules						Mental Arithmetic				
	A	B	C	D	E		A	B	C	D	E		A	B	C	D	E
A	—	·43	·34	·44	·25	A	—	·51	·50	·45	·52	A	—	·67	·67	·52	·56
B	·43	—	·45	·74	·75	B	·52	—	·45	·54	·62	B	·67	—	·55	·43	·70
C	·34	·45	—	·69	·50	C	·50	·45	—	·29	·64	C	·67	·55	—	·40	·51
D	·44	·74	·69	—	·57	D	·45	·54	·29	—	·29	D	·52	·43	·40	—	·57
E	·25	·75	·50	·57	—	E	·52	·62	·64	·29	—	E	·56	·70	·51	·57	—
	Average, ·52						Average, ·48						Average, ·55				

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[1] Spearman and Hart, this *Journal*, 1912, v. 55. See also Webb, "Character and Intelligence," *op. cit.*, 36; Carey, *op. cit.*, 172.

[2] Nunn, *Exercises in Algebra*, 511.

[3] The writer has followed Dewey in this use of the term 'suggestion.' There is something casual about the way in which the key-idea to the problem comes into the mind. Dewey, *How we think*, 26, 34; Nunn, *Education*, 47.

in this direction, his activity is of the nature of play[1], the expression of his surplus mental energy. But in a genuine difficulty there is no certainty that fruitful suggestion will occur. It implies, as Dewey says, a "leap into the unknown," for "thinking is primarily something which happens to us."

When suggestion comes to the child, he must receive it critically. The implications of the suggestion must be reviewed in relation to the existing difficulty, and the decision reached whether all the conditions of the situation are congruent with the suggestion or not. A common tendency of the child is to rely upon crude association, the 'part-workingness' of an experience. He is misled by a mere word, or by the partial resemblance of the new situation to an old one. Because of the element of chance about mental suggestion, and the undeveloped condition of the self-critical powers of the young, it is always well to test several times in order to obtain an approximately accurate estimate of ability in arithmetical problems.

It has been suggested[2] that each child probably has a definite and discoverable coefficient of ability in such subjects as problematic arithmetic, that this may at first be obscured by temporary accidental factors, but that, after a number of tests, disturbing conditions will tend to disappear, and, eventually, the particular ability, freed from them, will stand revealed in its true magnitude. This may or may not be the case in the simplified tests of the psychological laboratory. In problematic arithmetic, however, such a speculation is extremely difficult to test. It would be necessary to prepare sets of problems of uniform or uniformly graduated difficulty, and to set these to class-groups under similar examination conditions. In that case, if the speculation were in accordance with fact, the correlations between the later members of a series would, as containing the central 'problem' factor in greater purity, tend to approximate to unity, and a hierarchical arrangement of coefficients would appear. The present tests, given under conditions which could not be made ideal, may be challenged as not conforming to the required conditions. In so far as they go, however, they give little support to the 'steadiness' view. The brief analysis that has been given of the mental factors concerned in working problems will not favour the idea that the child will ever develop 'steadiness' in such a field as this. The great variations which occur between test and test are clearly revealed by the reliability coefficients of the tests, *i.e.* the averages of the inter-correlations, which are shown in Table X.

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[1] Nunn, "Play," *Educ. Times*, Nov. 1912; see also Nunn, *Education*, 79.

[2] An article by Winch describes this view. See *J. Phil., Psychol. and Sci. Meth.*, 1911, 375, 376.

Table X.

Reliability coefficients for higher forms of arithmetic.

Standard	Problems	Rules	Mental Arithmetic
7	·52	·48	·55
6	·49	·41	·68
5	·46	·60	·49
4	·65	·47	·71
3	·65	—	·69

It might by some be thought probable that the two school subjects, arithmetical problems and composition, both of which depend on the capacity of the child for doing independent work involving reasoning, would show on these grounds high correlations. This is, however, not found in a highly marked degree. The two exercises have such divergent elements that proficiency in the one may well accompany mediocrity in the other. There is certainly in this investigation no ground for contesting the modern view that, particularly with children, reasoning power tends to be specific. But neither is it denied that composition and arithmetical problems both take a high place as tests of intelligence. There is a strong probability that the child who excels in both is of high general mental calibre.

We are now in a position to examine the relations between the various branches of arithmetic. Their corrected correlations have been worked out, and a typical illustration of the results appears in Table XI.

Table XI.

Results for Standard 4 (corrected correlations), illustrating the dual character of arithmetical ability.

(Reliability coefficients in brackets.)

	Probs.	Rules	Ment. Arith.	Add.	Sub.	Mult.	Div.
Probs. ...	(·65)	·89	·86	·44	·52	·48	·71
Rules ...	·89	(·47)	·77	·43	·47	·60	·68
Ment. Arith.	·86	·77	(·71)	·47	·53	·51	·69
Add. ...	·44	·43	·47	(·67)	·80	·81	·71
Sub. ...	·52	·47	·53	·80	(·67)	·77	·73
Mult. ...	·48	·60	·51	·81	·77	(·76)	·78
Div. ...	·71	·68	·69	·71	·73	·78	(·70)

An examination of the tables shows that in each class arithmetical ability tends to be represented in two main sub-divisions:

(i) the power to compute with ease and readiness;

(ii) the power to do arithmetical work which involves the application of higher forms of mental activity than are required for (i). Under this heading are included problems, rules, and mental arithmetic.

Table XII summarises the results of all the classes, one of which has been shown more fully in Table XI. In all the five classes tested[1], the evidence is uniformly in favour of the dual character of arithmetical ability. The present investigation thus, up to a certain point, tends to support those of previous researchers, who have not found very high correlations between the fundamentals and arithmetical reasoning. It is therefore necessary to guard against the assumption that the power of facile calculation, to which great importance is attached in these days of commercial enterprise, will necessarily form part of the mental equipment of a child, whose arithmetical attainments may be from other points of view quite satisfactory. There is every justification for some small weekly allotment of time for separate practice in the bare elements of computation with the object of promoting speed and accuracy.

Table XII.

Standard	Average correlation between problems, rules, and mental arithmetic	Average correlation between the fundamentals	Average correlation between (i) fundamentals and (ii) problems, rules, and mental arithmetic
7	·83 (highest ·93) (lowest ·73)	·78 (highest ·90) (lowest ·64)	·47 (highest ·86) (lowest. ·24)
6	·61 (highest ·70) (lowest ·52)	·67 (highest ·96) (lowest ·35)	·19 (highest ·57) (lowest ·30)
5	·78 (highest ·85) (lowest ·73)	·73 (highest ·88) (lowest ·60)	·48 (highest ·73) (lowest ·06)
4	·84 (highest ·89) (lowest ·77)	·77 (highest ·81) (lowest ·71)	·54 (highest ·71) (lowest ·44)
3	·72 (highest ·72) (lowest ·72)	·75 (highest ·83) (lowest ·55)	·51 (highest ·70) (lowest ·29)

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[1] Standard 6, a small group of 25, happened to contain two or three lads, who were unusually good in computation, but who were otherwise poor in arithmetic. It was altogether a difficult standard, from the point of view of the experimenter. On the whole, however, it exhibited the general tendency.

Of the three higher forms of arithmetic, mental arithmetic appears to have the highest relation to computation. This is a perfectly natural result, for though intelligence was required to cope with the varying situations presented in the succession of questions, much of the work consisted of mere calculation. In Standard 7 appears the exceptionally high corrected correlation of ·86 between mental arithmetic and short division. In fact, in most cases, division, the hardest of the fundamentals, has the highest relation of them all to the more intellectual aspects of arithmetic.

In the category of appeal to intelligence, we might arrange the subject-matter of arithmetic thus: (1) Problems, (2) Rules, (3) Mental Arithmetic[1], (4) Division, (5) Subtraction, (6) Multiplication, (7) Addition. Of these forms, those which seem to have the greatest prognostic value are mental arithmetic, and short division. That is to say, the order of merit of a class in either of these two subjects appears likely to be very nearly related to the order of merit for arithmetic taken as a whole.

A comprehensive question arises. Arithmetic, as we have seen, is apparently divided into two sections, with some degree of dissimilarity. Is there any bond between these other than general educational ability—a factor which may be taken as operative in all school work, and the existence of which is indicated crudely by the fact that, with ordinary age- or class-groups of children, correlations in school subjects are usually positive? Is there a specific mental factor operating in all forms of arithmetic, that is, is success in one form of the subject to a special degree symptomatic of success in all?

In order that some answer might be given to this question, tests in composition, spelling, reading, geography, and history were included in the survey. In utilising their results, several methods, more or less satisfactory, are available. In the first place, if the correlations between the various branches of arithmetic are higher than those between any of these and the other subjects of the curriculum, the evidence is in favour of the existence of a special kind of ability common to all arithmetic. In Table XIII, column A shows the average correlation between com-

Table XIII.

Standard	A	B	C
7	·47	·29	·46
6	·19	·0	·29
5	·48	·26	·51
4	·54	·05	·35
3	·51	·26	·46

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[1] In the form in which it was given.

putation and the higher forms of arithmetic, column B between computation and the subjects of the curriculum outside arithmetic, column C between these latter subjects and the higher forms of arithmetic.

In every case, it will appear that computation bears a much higher relation to the other arithmetical processes than it does to the other subjects tested. On the other hand, there is certainly a distinct resemblance between columns A and C, which suggests a further field of inquiry. Similar results might well encourage the belief that there is only the common bond of the central intellective function exhibited in these two columns. But in that case, column B might be expected to show correspondence with the other two columns. This however is not the case. Computation has in general little affinity for anything outside arithmetic. Hence, we are impelled to interpret this relationship as evidence distinctly in favour of Burt's suggestion that there is an essential unity in arithmetical ability.

It is theoretically possible, having given the results of three tests, *a*, *b*, *c*, to determine to what degree any of them is saturated by the same general factor, it being understood that there is no specific relation between the mental characters tested by *a*, *b* and *c*. The formula

$$r^2_{ae} = \frac{r_{ab} \cdot r_{ac}}{r_{bc}},$$

by which the degree of saturation of *a* with *e*, say educational ability, may be estimated, is derived by elementary algebra[1] from Yule's formula.

The course adopted was to divide the subjects into three groups,—(1) arithmetical, (2) English, including composition, spelling, reading, (3) geography and history. The division is on the whole justified by the results of the tests. To apply the formula, one subject was selected from each group, the process was repeated with other selections as often as possible, and the average was taken. Table XIV shows the final results for Standard 5. Line A at the bottom shows the degree of saturation of each subject with general educational ability. Secondly, for checking purposes, a modification of a method used by Burt[2] was employed. The results of this are shown in line B.

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[1] If there is no specific relation between the tests

$$r_{ab.e} = 0,$$

$$\therefore\ r_{ab} = r_{ae} \cdot r_{be};$$

also

$$r_{ac} = r_{ae} \cdot r_{ce},$$

and

$$r_{bc} = r_{be} \cdot r_{ce};$$

$$\therefore\ \frac{r_{ab} \cdot r_{ac}}{r_{bc}} = r^2_{ae}.$$

[2] Burt, *Educational Abilities*, 53.

Thirdly, use was made of the various intelligence tests to obtain two measures of the influence of the central intellective function, as nearly as possible with no specific relation to each other. By correlating each of these with the various subjects of the curriculum and with each other, the formula

$$r^2_{ag} = \frac{r_{ab} \cdot r_{ac}}{r_{bc}}$$

could be used, *b* and *c* representing the measures of intelligence, *a* the school subject, *g* general intelligence, a more suitable expression than *e* to use for tests which lay outside the field of school instruction. For the intelligence tests represented natural capacity rather than the effects of training. This method has the advantage of offering an independent estimate of ability, against which the school subjects may be independently measured. As in the other cases, however, it was not possible to eliminate entirely specific factors, or sampling errors. It is to be noted also that the particular intelligence tests employed, suitable as they were for group use, have all a linguistic character, and consequently tend to betray a special affinity for the English subjects.

To obtain the two separate measures of intelligence, most of the tests given were called into requisition, every care being taken to avoid the pitfall of the common specific factor. The two measures were not made up in the same way for each class. For Standard 7, the mutilated text, and opposites tests were used. For Standards 5 and 6, the re-arrangement of disordered sentences was taken for one measure, and a combination of opposites and analogies for the other. Opposites form merely a special case of analogies, an exercise where it is required to find a term which stands in a certain relation to another term. In all cases of combinations of intelligence tests, the question of the suitable weighting of marks was considered. For Standard 4, a combination of the earlier and later forms of the completion test was used for the first measure of intelligence, and for the second a combination of the opposites and disordered words tests. Though these two last-mentioned tests do not appear to show any specific relation, there seems to be no objection to combining them for an independent measure of intelligence. A similar procedure was adopted by Webb[1]. In Standard 3, the opposites test was taken as one measure of intelligence, and a combination of the disordered sentence test and the supplementary completion test for the second. Use was made of the two measures of intelligence in each case to find the degree of saturation of each school subject with *g*. The final results appear in line C of Table XIV. All of these estimates of the influence of a central factor are open to objection, but, with few exceptions, they are substantially in accord.

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[1] Webb, "Character and Intelligence," *op. cit.*, 38.

Table XIV.

Complete table of correlations for Standard 5.
(Reliability coefficients in brackets.)

sp. = speed in computation
* acc. = accuracy in computation
pr. = problems
rul. = rules
men. = mental arithmetic
cp. = composition
spl. = spelling
rd. = reading

his. = history
geog. = geography
I_1 = first intelligence measure
I_2 = second intelligence measure
A = correlation with educational ability
B = check
C = correlation with general intelligence

	sp.	* acc.	pr.	men.	rul.	cp.	spl.	rd.	his.	geog.	I_1	I_2
sp.	(·72)	·16	·26	·47	·61	·07	·42	·40	·19	·20	0	—
* acc.	·16	(·31)	·38	·29	·20	·32	·17	·31	– ·05	·20	—	—
pr.	·26	·38	(·46)	·75	·73	·55	·44	·45	·47	·74	·77	·58
men.	·47	·29	·75	(·49)	·85	·27	·55	·49	·59	·54	·52	·47
rul.	·61	·20	·73	·85	(·60)	·51	·39	·60	·47	·62	·47	·35
cp.	·07	·32	·55	·27	·51	(·74)	·55	·63	·77	·51	·46	·74
spl.	·42	·17	·44	·55	·39	·55	(·87)	·57	·50	·47	·50	·74
rd.	·40	·31	·45	·49	·60	·63	·57	(·79)	·50	·44	·47	·61
his.	·20	– ·05	·47	·59	·47	·77	·50	·50	(·71)	·88	·49	·64
geog.	·19	·20	·74	·54	·62	·51	·47	·44	·88	(·83)	·58	·51
I_1	·0	—	·77	·52	·47	·46	·50	·47	·49	·58	(·81)	·64
I_2	—	—	·58	·47	·35	·74	·74	·61	·64	·51	·64	(·67)
A	·32	—	·74	·68	·72	·64	·72	·74	·75	·76		
B	·35	—	·72	·67	·71	·77	·69	·72	·84	·77		
C	·0		·83	·62	·50	·74	·76	·66	·70	·68		

* uncorrected correlations of the amalgamated totals.

The results of A have been selected as probably affording an approximately accurate expression of the influence of general educational ability. Yule's formula for partial correlation has then been used to eliminate this in order to determine specific correlations between the various branches of arithmetic. The results appear in Table XV.

Without an exception, when the general factor has been eliminated, positive correlations, frequently high, are found between the different forms of arithmetic, indicating that, on the whole, there is a special form of ability which functions in terms of number instead of words. The results of the application of the method of partial correlation therefore corroborate those of the simpler Table XIII, which shows that mental arithmetic, rules and problems are closely connected with each other, and that skill in computation is allied to these, but not, as a rule, to other subjects of the school curriculum.

Table XV.

Specific correlations in arithmetic.

C = computation P = problems
R = rules M = mental

	Standard 7			
	C	P	R	M
C	—	·29	·48	·25
P	·29	—	·64	1·00
R	·48	·64	—	·87
M	·25	1·00	·87	—

	Standard 6			
	C	P	R	M
C	—	·23	·44	·04
P	·23	—	·60	·29
R	·44	·60	—	·27
M	·04	·29	·27	—

	Standard 5			
	C	P	R	M
C	—	·04	·36	·58
P	·04	—	·50	·42
R	·36	·50	—	·71
M	·58	·42	·71	—

	Standard 4			
	C	P	R	M
C	—	·67	·63	·60
P	·67	—	·74	·81
R	·63	·74	—	·62
M	·60	·81	·62	—

	Standard 3		
	C	P	M
C	—	·27	·59
P	·27	—	·59
M	·59	·59	—

Brown[1] has stated that "the balance of evidence seems to be in favour of the existence of a special capacity or faculty, underlying mathematical ability, distinct from, and with no essentially close connexion with other forms of intellectual capacity." Burt, as we have seen, expresses the belief that there exists a special form of ability, which manifests itself generally in arithmetical work.

The present survey offers additional evidence in favour of these two views. The connexion between the various forms of arithmetic, even when the effect of the many obvious common elements has been reduced to a minimum by the system of marking employed, is so strong as to suggest a specific factor in addition to general educational ability.

It is impossible, therefore, to accept the view that arithmetical ability is a 'myth.' The 'non-focal' views that have been reviewed here are not supported by the survey. The general trend of its results is to show that there is a single specific factor, operative in all forms of arithmetical work, however unlike these may appear to be in psychological appeal.

V. SUMMARY OF CONCLUSIONS.

1. The variability in form of the individual child is apparent in all kinds of mental testing. It is particularly pronounced in problematic arithmetic.

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[1] Brown, *Child Study*, March 1913, 26.

2. Accuracy in computation, as judged by the frequency of error, cannot usually be measured with a satisfactory degree of reliability.

3. Speed and accuracy in arithmetical work are positively correlated.

4. There appears little evidence to support the theory that 'steadiness' in ordinary school performances may be expected after a number of tests.

5. The intelligence and the knowledge aspects of arithmetic, exemplified in problems and rules respectively, are highly correlated. The fundamentals are highly correlated with each other, but less closely with problems and rules. There thus appears a certain degree of duality in arithmetical ability.

6. Owing to the influence of general educational ability, all school subjects have a tendency to be positively correlated in ordinary class groups. There might therefore be a temptation to ascribe to this general factor the correlations found between computation and the higher forms of arithmetic. Educational ability is however inadequate as an explanation.

7. The evidence of the survey is distinctly in favour of the proposition that there is a single specific mental factor, operative in all kinds of arithmetical work.

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THE CAUSES OF THE WAR IN CURRENT TRADITION

BY W. T. WAUGH.

IN a recent public examination, the History paper contained the question, "Why did the German Government provoke war in 1914?" The candidates were boys and girls of fifteen or sixteen, and came from various parts of the north and midlands. Their object was to qualify for admission to a teachers' training college, and it may be presumed that they were among the most intelligent and successful pupils in their respective schools.

Very few candidates attempted the question, and those that did generally limited themselves to a bald summary of what they believed to be the events of July and August, 1914. The answers were mostly inaccurate and uninstructive, and several revealed nothing but astonishing ignorance. Two, or three, however, have some claim to the attention of those interested in the systematic study of testimony. They should, in my opinion, be regarded as products of oral tradition, for in 1914 the writers were not old enough to understand the momentous events which were taking place, and their present knowledge of them is probably based for the most part on what they have heard from teachers or parents.

The three following passages seemed specially instructive, and worthy of being quoted at length. Each is from a different candidate:

(1) "Germany thought that if she broke what was called the 'Scrap of Paper,' she would be able to attack England more easily. The treaty which she was breaking said that Germany could not use Belgium as a means of transporting troops in time of war. The breaking of the treaty annoyed France as well as Belgium so she allied with Belgium against Germany....England saw that if she did not help France and Belgium, Germany would more easily be able to attack her when she had conquered France, so England joined France and Belgium. These three allies were known as the Triple Entente." The sequel concerns the murder of the Archduke Franz Ferdinand; it is generally true to fact and calls for no special remark.

(2) "Kaiser Wilhelm II...wished to strike at England and so attacked Belgium and took Alsace-Lorraine—two states which were given to Germany at the close of the Franco-Prussian war, 1871, but which being French states were given back to France later on, by the wish of the

people. After ravaging Belgium the Germans attacked the northern borders of France. England was allied with France and so went to her aid. This brought about the thing that Germany wanted, *i.e.*, war with England."

(3) "The Serbian Crown Prince was murdered by an Austrian, so Serbia declared war on Austria. Russia was bound to help Serbia since it was to her own advantage, and Germany stood by Austria. Then France had to join in with Serbia and Russia because of her long and bitter hatred of Germany, thinking she would retaliate for the Franco-Prussian war of 1870. To invade France, Germany would have to cross through Belgium, and though she was warned not to by the other powers she still persisted in doing so, and England was in honour bound to protect Belgium." What follows reveals a fairly accurate knowledge of the principal treaty which guaranteed Belgian neutrality.

These extracts, with the evidence afforded by other papers, show plainly that, even before the war was formally ended, the rising generation had utterly confused notions of how it began. The political significance of this, though serious, cannot be discussed here. The problem for the psychologist is to analyse and explain the distortions of fact which mark the three passages quoted and indeed all the answers to the question. Their main source is evident—namely, a desire, unconscious it may be, to make things easy, and in particular to bring the events narrated into obvious harmony with certain fundamental assumptions. It was generally taken for granted by the candidates (*a*) that Germany was in the wrong, (*b*) that Germany was the aggressor, (*c*) that Germany's main object was the overthrow of England. These three postulates were clearly accepted by the writers of extracts (1) and (2) quoted above; the writer of extract (3) accepted postulate (*a*) and, so far as Belgium and England were concerned (*b*) also, but does not seem to have been affected by (*c*). A very interesting illustration of the influence of postulate (*a*) is afforded by the remarkable inversion of the facts that occurs in the first sentence of extract (3). The writer knew that the murder of the heir to a European throne marked the beginning of the crisis. She had no doubt that Germany was the villain of the drama that followed. Therefore the murderer must have been, if not actually a German, a subject of one of Germany's allies in the war, and the victim a subject of one of her enemies. Similarly, the astonishing statement about Alsace-Lorraine in extract (2) seems to be due to the influence of postulate (*b*). If Germany was the greedy aggressor, how came it that, at the end of the war, she had to surrender some of her own territory to a state which (*ex hypothesi*) had been fighting in self-defence? Why, because Alsace and Lorraine had belonged to France at the outbreak of war, and, like Belgium and north-east France, had been overrun by the German invaders. But the writer could not forget that Alsace and Lorraine had been annexed by Germany in 1871, and she therefore had to invent a subsequent cession of the provinces to France, the principle of self-determination being ingenuously called in to lend an air of verisimilitude to the story. It is not surprising that she was confused, for the terms of peace are supposed to have involved "no annexations," and in public discussion Alsace and Lorraine have commonly been treated as if they were on precisely the same footing as Belgium.

The third assumption—that Germany's main object was the overthrow of Britain—exercises a strong influence on extracts (1) and (2). The writers evidently felt that Germany's principal motive must have affected her conduct of the war from the outset, and they accordingly adduce it to explain the violation of Belgium neutrality, though how this was expected to promote the destruction of Britain is not made clear. It appears, in fact, that none of the candidates really understood the significance of the invasion of Belgium. Of those cited above, the first two failed to realise that it was the immediate cause of Britain's entry into the war; and the third, while grasping the idea of neutrality and the obligations of Britain towards Belgium, was led away by her ignorance of geography from the true explanation of Germany's action. But in each case, it should be emphasized, the version presented is simpler than the actual facts. In the first two extracts Germany invades Belgium because such a course will directly serve her object of destroying Britain; in the third she does so because there is no other way of getting at France. Either explanation is much easier to understand than the strategic plans of the German General Staff.

The same tendency towards simplification is revealed by the reluctance with which the candidates touched upon the predisposing causes of the war. Few of them felt sure of their ground when they ventured further back than the murder of the Archduke, and most tried to indicate that the events of the last few weeks before the war were sufficient to account for what followed. The writer of extract (1), indeed, goes so far as to make the formation of the Triple Entente a consequence of the invasion of Belgium.

Of course some of the misapprehensions and omissions of the candidates are due to merely fortuitous influences and possess no interest for the scientific student of evidence. The cynicism of extract (3) is probably unconscious, the writer, one may suggest, having imperfectly grasped a lesson given by some teacher of judicial temperament. Very interesting, on the other hand, is the treatment of the "scrap of paper" phrase in extract (1)—a complex instance of a process familiar to all historians, whereby a word or phrase used originally in scorn or irony loses its sting and acquires an inoffensive or even honourable significance[1]. The disappearance of Russia from the Triple Entente—another notable feature of this passage—is natural in view of recent events; it is a further example of harmonization, only in this case the criterion is the situation at the time when the examination was held. It is, however, astonishing that Italy is not named as a party. Apparently the writer knew more about the course of the war than a first reading of her answer might lead one to suppose—a wholesome reminder that most people's memories are capricious to a degree that is seldom realised even by trained investigators of evidence.

It may be urged that the interest of the passages quoted is vitiated by the fact that they were written by children, who, however intelligent, lacked the experience and knowledge essential to a comprehension of international politics, and that grown men and women, even though their information had been derived from precisely the same sources, would have answered the question very differently. That may well be; but it is extremely doubtful whether they would have answered it any better. For there is much to be said for the view that boys and girls in the higher forms of our secondary schools are in general keener on knowledge, more "gleg in the uptake," more critical, and better-informed on matters outside their everyday experience, than the majority of their adult compatriots, while there can be no question as to the normal superiority of their memories. And, in comparison with most of the witnesses on whom, in the last resort, our knowledge of history depends, they have all the advantages just enumerated, together with a faculty of expression which, until the present age, was possessed by few. If, in short, allowance ought to be made for the youth of the writers whose work we have been considering, it is because, as witnesses, they are likely to be above the average.

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[1] Well-known examples are the terms Whig, Tory, Methodist, Quaker. But their development is relatively simple, and I cannot recall a case quite parallel to the one before us. The phrase was first used by Bethmann-Hollweg in contempt, then taken up by Germany's enemies and used in irony, and now, it appears, there is a possibility of its becoming regarded as the name by which, even before the war, the Treaty of London of 1839 was commonly known.

A THEORY OF BINAURAL HEARING.

By HENRY J. WATT.

In recent years notable additions to exact knowledge of the binaural functions of hearing have been made, especially by O. Klemm[1]. The evaluation of such results, however, is greatly impeded by the lack of any promising basis upon which to construct a theory of them. In my *Psychology of Sound*[2] I ventured to propound a theory of the direct basis of binaural localisation that arose inevitably out of the interpretation of pitch-differences as ordinal or positional. This theory, as I showed, seems to meet satisfactorily both the main facts and the introspective descriptions of binaural processes. I should like now to show how it is confirmed by various recent delicate observations and to comment briefly on certain outstanding problems.

My theory may be essentially described as the application to hearing of Bernstein's theory of single touch from double contact[3]. As a result of continued consideration of the problems of hearing I am now inclined to look upon the auditory field as the exact psychical replica of its anatomical structure. This view of course, has the merit of bringing hearing into line with the other senses and of satisfying the general postulates of psychophysical parallelism, for the cerebral-psychical parallel at least. As von Frey suggests for the skin sense[4], it seems probable in hearing that a small number of nerve fibres may connect the receptor with the cortex and yet in the process each receptive element may be unambiguously related in the normal organism with a central spot and so receive an individual positional attribute. In its one aspect (longitudinal or 'musical') the anatomical area is a very long strip of receptive elements. In the other aspect (transverse or

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[1] G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen. 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 226–70. O. Klemm, "2. Versuche mit einem monotischen Beobachter." *Ibid.* 497–505. O. Klemm, "3. Ueber den Anteil des beidohrigen Hörens." *Arch. d. ges. Psychol.* 1918, XXXVIII. 71–114.

[2] H. J. Watt, *The Psychology of Sound.* Cambridge, 1917.

[3] T. Thunberg, "Physiologie der Druck-, Temperatur-, und Schmerzempfindungen." *Nagel's Handbuch der Physiologie des Menschen.* 1905, III. 720. C. S. Myers, *Textbook of Experimental Psychology*, ch. XVII. Cambridge, 1911.

[4] M. v. Frey, "Beobachtungen an Hautflächen mit geschädigter Innervation." *Ztsch. f. Biol.* 1914, LXIII. 350 f.

'locational') it is very narrow. The binaural locational function is concerned primarily with the transverse aspect alone.

This consists generally of only some three to five terminals or, if we allow for their alternating arrangement, of perhaps only two or three terminals at any transverse section. Now in Bernstein's theory each terminal is assumed to give centrally a little area of excitation most intense at the centre and decreasing gradually outwards to a subliminal margin. To each such central excitation a spot of sensation corresponds which, as I take it, differs in its ordinal or positional attribute from the spot derived from a neighbouring terminal. This small transverse row of spots may be supposed to yield on full stimulation a single graded mound of excitation with a maximum at its centre.

For the purposes of binaural hearing my theory now assumes that these combined uniaural mounds, in so far as they are related to transverse sections of the longitudinal series of either ear at similar points, again combine by overlapping, namely when the two ears are stimulated simultaneously at these transverse points by any sound. The overlapping of course is not complete, but only partial as in Bernstein's familiar figure of single touch from double contacts. In my *Psychology of Sound*, I showed how the main binaural functions could be deduced from suitable variation of the relative intensities of the uniaural components and their summation. I shall therefore limit myself now to showing how Klemm's recent observations confirm and extend my theory.

Like Baley[1] Klemm finds that the binaural sound has a greater volume than the uniaural[2]. The resultant spot in touch is more extensive than the single one. Similarly the diotic sound is much louder than the monotic, four times as loud by estimation. This is more than is required by my theory, which would commonly yield duplication, if not rather more when we take into account the difficulty of judging accurately what is 'double.' Klemm's reckoning, however, lies on the safe side for me, for there may well be some mutual facilitation, psychological as well as physiological, between the ears. The two ears will bring into psychical prominence the best that either contains, as Dawson showed for vision[3]. Accordingly the diotic sound seems fuller and purer like an organ tone while the monotic seems thin, as many observers have stated. These relations hold for diotic tones, when the excitation in both ears is the same.

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[1] S. Baley, "Versuche über die Lokalisation beim dichotischen Hören." *Ztsch. f. Psychol.* 1915, LXX. 347–372.

[2] O. Klemm, "3. Ueber den Anteil des beidohrigen Hörens." *Arch. d. ges. Psychol.* 1918, XXXVIII. 72.

[3] S. Dawson, "Binocular and Uniocular Discriminations of Brightness." This *Journal*, 1913, VI. 78–108.

In dichotic hearing (when different sounds are led to either ear) similar relations should hold so far as the differences in the longitudinal aspects of the tones allow. But this will be complicated not only, as Baley found, by the possible distinction of these different monotic tones[1] but also by their overlapping in the central (diotic) part of the transverse field of hearing. This overlapping will naturally produce the same sort of hiding or involution of a higher tone by any lower (and even weaker) tone as is familiar monotically. This hiding I have explained in my theory of the longitudinal functions of the auditory field[2].

Klemm also finds that for the discrimination of monotic intensities a higher (but still comparable) difference is required than for the binaural function that is based on intensive difference. This seems to fall into the class of such facts as the greater displacement required for the apprehension of direction or for the distinction of end positions than for mere motion or for mere increase of extent, for the designation of direction of change generally than for mere change-at-all or for the first signs of the integrative product of that change. It is clear that for any and every comparison of monotic intensities the usual unity of the diotic effect must somehow be broken up, *e.g.* by partially or wholly successive presentation. For the combined diotic effect can only be read as "maximal intensity at a certain point of the transverse binaural series." Localisation involves no comparison of the components but is a resultant unity read as it stands.

It may be noted at this point that any ratio of intensities can be made to give an indefinite series of absolute resultants. The maximum of the resultant and its gradients may be raised or lowered without any displacement of its position in the transverse series. A basis for the discrimination of the distance of a sound from the head (or its 'depth') is thus provided. The important experimental analyses of Klemm shew that intensity plays an important part in the impression of the distance of sounds[3]. But it obviously cannot be the only factor, for the ratio of monotic intensities cannot remain constant for all positions on a line (drawn in any direction outwards from the centre of the head) that lies outside the median plane. The binaural ratio would therefore give a starting point lying on a base line ('between the ears'). The absolute intensity of the maximum as at this point would 'project' this base line far out from the head, whereby the binaural positions would now become a point lying in a certain direction from the head. One or more other factors, amongst which differences of blend of partials is commonly held to be the most important, would be required to distinguish between front and back, up and down, and between the ambiguities contained in the above two factors.

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[1] H. J. Watt, *The Psychology of Sound*, 183 f. Cambridge, 1917.

[2] *Ibid.* 71.

[3] *Cf.* G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen. 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 247 f.: "The usual complex limen for distance S_s is dependent on the limen for intensity S_i' and the limen for pure distance S_s'. If S_i' were greater than S_s', S_s would

As regards the possibility of a pure distance factor other than blend of partials that Klemm's work[1] seems to imply, it may be suggested at this point that bone conduction might conceivably produce waves in the excitation of the cochlea in the path of its passage through that organ in its incoming directions. Only the excitation by bone conduction that falls into line with the normal stimulation of the ear by the small bones would produce the appreciation of tones in their full ordinary manner[2]. But this full excitation might possibly be criss-crossed in all directions by anomalous ripplets, as it were, that would produce a colouring of sound that we could learn to recognise but not perhaps to separate off analytically. A basis for the perception of direction of this kind would be available for purely monotic hearing, when the mind is forced to rely on the subtlest indications. Intracranial

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presumably coincide with S_e'; then intensity would have very little meaning at all for the appreciation of depth. If S_e' were incomparably greater than S_i', then again S_t would nearly coincide with the latter; and the impression of changed depth would have to be evoked when S_i' is reached, since intensity would now alone be responsible for the judgments of distance. Now in reality S_e' is notably greater than S_i'; S_t must accordingly lie between S_i' and S_e'. If the distance has changed only by the amount S_i', something is somehow still lacking for the pure impression of change of depth. On the other hand, the limen S_t indicates that with great enough change of intensity the impression of changed distance can be produced despite the resistance offered by the marks of depth that are associated with constancy of distance. The value of S_t will be the greater the smaller S_e' and the greater S_i; the marks of depth that come into operation in the limen S_e' seem to protect the apprehension of distance against the illusion (*Täuschung* = *t*) induced by the change of intensity.

And this psychologically reasonable relation we find confirmed in the figures, in fact in the various cases the values S_t are approximately equal to $S_i : S_e'$" (*cf. ibid.* 255 f.). It is clear, of course, that the judgment of distance on the basis of pure intensity is as open to monotic as to diotic observations (*cf. ibid.* 111).

[1] *Op. cit.* pp. 226–270.

[2] H. J. Watt, *The Psychology of Sound*, 168 f. Cambridge, 1917.

localisation could hardly be founded on this basis, however, as it appears not after learning, but suddenly and forcibly to the observer's surprise.

Nothing need be added to my previous statements regarding the correspondence between the capacities of binaural localisation and the variations made possible by the relative intensities of the overlapping mounds of the theory. As Klemm says[1]: "Certain as is the influence of intensity difference outside the sagittal plane, it is just as certain that by itself it can only procure distinction of the right and left hemisphere and a perception of some degree of departure from the sagittal plane, but not the apprehension of a particular direction." That is just the deduction I made from my theory.

But it may be noted that no abnormal alteration of the natural ratios of intensities that is permanent (*e.g.* owing to wax plugs in the ears or to partial deafness) will noticeably impair the binaural function for ordinary use. We learn to correlate the position on the transverse series that is actually caused by locus in the median plane with our other positional experiences (*e.g.* visual, motor, ideational) relating to that plane, and we make our auditory space correspondingly. Thus in a relatively monotic observer the median plane would not be displaced towards the worse ear as in the case of artificial or temporary deafness on one side[2]. The ratio of intensities actually emanating from the median plane, though very far removed from equality, would be correlated with the median plane as otherwise given. And so the deafened observer would in course of time learn to localise with considerable accuracy. But the result would be subject to all the individual variations and anomalies of such acquired powers.

This is merely a corollary to my thesis that bi-systemic functions such as the binaural and binocular are still only ordinal or concerned with form, and not spatial. But the binaural position is inherently positional from the first, whereas the others (absolute intensity of binaural maximum and blend of partials) can only become signs of positions by being correlated with positions otherwise given.

Very strong support is given to my theory by Klemm's experiments on the variation of the time relations of the two components[3].

(1) "With the smallest time differences the sound stayed in the middle, but as compared with the effect of simultaneity it was broadened

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[1] G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen. 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 254.

[2] O. Klemm, "2. Versuche mit einem monotischen Beobachter." *Ibid.* 501.

[3] O. Klemm, "3. Ueber den Anteil des beidohrigen Hörens." *Arch. d. ges. Psychol.* 1918, XXXVIII. 106 ff.

in a peculiar way. The sound remained unitary and this extension seemed to make evenly for both sides; it no longer lay so neat and small in the median plane as in the case of simultaneity." This change was the only sign of succession to be noticed, whether the left or right ear came first or not.

(2) "With the next step the broadening of the sound was attended by a displacement from the median plane towards the side of the earlier ear." The sound was still unitary, but the earlier ear could now be distinguished by the observer in the shape of this displacement of position, not as a time phenomenon strictly.

(3) Finally: "The unitary sound broke up and the time difference came to consciousness. At first, however, there was an apparent movement from the earlier to the later ear,"—a sort of cinemato-acoustic illusion. The sound seemed to rush through the median plane[1]. This cinematic illusion yielded gradually to the impression of a pure double beat.

A clearer confirmation of theory than these notes provide could hardly be desired. We have here practically a parallel to the steps from one resultant touch to two distinct touches noted by Foucault[2]. The resemblance to the results of studies in visual and tactual movement is also obvious. The earlier ear gives the maximum at first to one side while the later ear pulls it over to the other. As there is a limit to motional rate, the smallest time-difference must at first give only a spread of the maximum and so a broadening. The excitation rises with a slight hump towards the side of the earlier ear. As the time difference is increased this hump will pull further out towards the earlier ear until it is at it, so to speak. Then the component for the other ear will pull the maximal point over towards its side, the farther the greater be the time-difference; for the second excitation will benefit by the facilitation afforded by the earlier stimulation. At a certain rate this will give optimal motion, while with greater time-difference there will be increasing time for the earlier ear to make its position felt as such and to be followed by the second as a distinct beat. Here again we see how motion implies differences of position though not the distinction of these positions as such. The times for these first stages until the greater part of the motion is lost should lie well within the time of auditory excitation increase. Thus we can understand how the third stage was found by Klemm to begin after 5 σ. It is just such results as these that allow us to estimate the time required for optimal interaural motion. When the motion is not so extensive as this but is to cover only a small arc of the horizontal circle round the head, as in Burtt's experiments where the successive positions of the sound were "a few centimetres apart"[1], longer time-intervals would be required. Burtt found the optimal interval to vary from some 10 to 50 σ with an average of 25 to 30 σ. So his results help to provide "evidence for the correlation of separate points in auditory space with separate regions of the sensory cortex" (p. 74). Thus we should expect to find the now fairly general laws of cinematic motion verified in the transverse aspect of hearing. So the total duration of the stimulus should not be long; for the earlier cessation of the first stimulus will allow it freer drainage into the later one. And if the stimuli are too intense, the increase of excitation will be too rapid. But a later intenser stimulus should be able to reverse the illusory movement, etc.

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[1] A. H. Pierce, *Studies in Auditory and Visual Space Perception*, 66. New York, 1901.

[2] M. Foucault, *L'année psychol.* 1911, XVII. 420 ff.

As for Weber's experiment with the two watches to which Klemm alludes[2], the difference noted between monotic and dichotic hearing does not prove that the central connexions of the two ears remain independent. The two watches are heard to coincide and separate rhythmically in monotic hearing because their sounds equally fill the monotic transverse volume. In dichotic hearing either watch can be heard separately by special direction of the attention, because then the combined field of hearing contains a central diotic part with a monotic part (beyond the overlap) on either side. Attention to one ear can fix on the corresponding monotic part and so get a basis for the adjustment of the attention to all the details of the presentation in question. This is quite in line with Baley's observations which I discussed in my *Psychology of Sound* (183 f.). We can see a basis here also for a sort of transverse timbre or blend of partials that could be correlated (without analysis) by 'experience' with positions given through other channels than hearing.

As for the pitch differences between the two ears I have nothing to add to my previous interpretation of them, which still seems to me as direct and satisfactory as one could expect. We must apparently consider these differences as the remainders left by the progressive approximation to perfect inter-adjustment and cooperation of the two ears, certainly not as anomalous departures from a perfection that is presupposed as primary. Klemm's (limited) statistics suggest a normal distribution about a maximal frequency of no difference at all. The imperfection of that elasticity of the basilar membrane or similar structure which my theory of the longitudinal functions of the auditory field rests upon, is not an unnatural or alien hypothesis. It is merely a corollary to any assumption regarding elasticity. Theories which treat pitch as qualitative must first leave the qualitative identity of the two ears (like pitch-quality in general) quite unexplained and must then assume that to some extent the qualities of the two ears are not identical. There is, moreover, no reason why minute pitch differences should make us hear double. That should only come about when the difference is great at any point and extends over many points. When one ear is badly mistuned (by disturbances in the middle ear perhaps), double hearing can be very distinct indeed, as I have myself experienced it.

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[1] H. E. Burtt, "Auditory Illusions of Movement—A Preliminary Study." *J. of Exp. Psychol.* 1917, II. 63–75.

[2] G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen. 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 81.

Klemm's experiments on the artificial inversion of the intensity difference[1] belong to the problem, not of the binaural function strictly (binaural position), but to that of binaural space. He himself suggests this by his comparison of them with the familiar experiments of Stratton on the inversion of visual space by the wearing of special glasses. To some extent his experiments demonstrate the presence of alternative effects of attention similar to those demonstrated by Wertheimer and others with visual motion. This implies that under certain circumstances, limited in scope no doubt, the attention is able to alter the sensory datum. How far in the case of hearing we have a real alteration, as in the accents of rhythm, or an apparent alteration due to the inertia of the recognition of alternation, or merely a change of correlation (which the very limited scope of absolutely primary intensity localisation suggests) is not quite clear. Klemm himself puts it down to the spatial misdirection of the complex of associative connexions[2], not to a change in the actual basis of auditory localisation. But, anyhow, these changes will be as real as in rhythm, illusions, motions, etc. And their basis will doubtless be found in an essential kinship of the attentional attitude with, or its ultimate origin out of, the sensory basis such as is altered by it. Such alterations by the attention do not of course imply any permanent obscuration of the primary basis of localisation. For unprejudiced observation can readily get itself into line with the latter again, as the experiments show. Some observers may not exhibit the illusion at all[3]. And any novelties or irregularities in the impressions dispel the distorting effect of attention at once.

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[1] G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen. 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 98 ff.

[2] *Cf. ibid.* 104.

[3] *Cf. ibid.* 105.

Numerous details in the functions of binaural hearing have still to be described exactly and explained. (One of the most surprising of these perhaps is the intracranial[1] localisation.) But I think my theory meets the known details of experimental investigations both intensively and extensively enough to be considered a probable theory of this otherwise (and as even Klemm leaves it) perfectly mysterious and unintelligible bi-systemic function. It has the merit, if I may say so, of being an extremely simple theory from which the whole range of facts can be easily deduced.

At the same time it adds considerable strength to my explanation of the other functions of hearing. The longitudinal aspect of the auditory field might conceivably be associated with a system of irreducible qualities, while the transverse yielded a series of positions. But that is hardly probable, one may surmise. Why two so different affiliations? Besides is not any systematic theory that adequately covers the main trend of facts now known and satisfies the general principles of psycho-physical parallelism, so giving free scope to the spatio-temporal theories of physiology,—is not such a theory preferable to none or to such as deal in vague assumptions regarding qualities or motor associations or the like that are incapable of analysis and complication and so offer no prospect of progress? If the value of theories is either essentially pragmatic or systematic, the answer can hardly be other than assent. The kinship of the longitudinal and transverse differences, moreover, is strongly supported by the fact that we can only localise differently pitched sounds in different places at once. Identically pitched sounds compound to a phantom sound whose apparent position depends upon the resultant intensity. There is of course no proof to be drawn from these relations, but the unity and clarity of explanation could hardly be said to lie with the qualitative theory of pitch. At the same time we see that as the musical dimension is abstracted from the locational so that tones seem to have only that volume which varies in them, namely length without breadth, so the transverse aspect is abstracted from the musical, so that the volume of a tone is never read as a length in space[2], nor as spatial, while the transverse position is. Both sets of positions, longitudinal and transverse, begin and persist as positions, but the transverse enters into integrations that make its positions spatial.

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[1] As such of course, not in its internal differences, which form a replica of the extra-cranial (*cf. ibid.* 88).

[2] *Cf.* Burtt, *op. cit.* 190.

PUBLICATIONS RECENTLY RECEIVED.

Psychology and Folklore. By Dr R. R. MARETT. London: Methuen & Co. Ltd. pp. ix + 272. 7*s.* 6*d.* net.

The author has collected in this book a number of his recent addresses, lectures and reviews, having enough in common (for the most part) to give them an effective unity. In nearly every article he dwells on the importance of Psychology, to the understanding of the social phenomena of Anthropology: a point at which, probably, most readers of this *Journal* will agree with him. Some instances of interesting psychological insight will be found at p. 137—on the emotions aroused by crossing a threshold; at p. 139—on the blood-covenant; and at pp. 216–7—on the operation of suggestion.

Generally Mr Marett complains of the prevalence of a too intellectualist method of interpreting savage life, as (for example) in Sir James Frazer's writings, and prefers the more emotional appeal of M. Durkheim (183). Undoubtedly the importance of emotional conditions in determining savage beliefs and institutions cannot be overrated; and savage emotions can in some sort (though perhaps very imperfectly) be appreciated by direct sympathy. Sympathy is necessary to one's own appreciation of the case—there is no other means; and Mr Marett has tried earnestly to enter into the mind and motives of the lowest culture. But when it comes to public explanation, the attempt to describe emotional conditions intellectualises them: explanation can only be intellectual. Our only way of conveying to others a lively sense of the emotions of a savage, and of how they work, is by exhibiting the ideas and practices which excite, or which are exited by them.

A question which brings this opposition to an issue is the definition of 'religion.' For Mr Marett religion is essentially an affair of emotion: it is the sense of 'sacredness' (158) interfused with certain social practices or institutions; for example (according to M. Durkheim) with Australian totemism (188). Sir James Frazer, on the other hand, defines religion as consisting of two elements—"a belief in powers higher than man and an attempt to propitiate or please them": which excludes totemism. But this is intellectualist: for such a definition decides whether an institution is religious or not "by the presence or absence of a certain concept" (188). In the passage quoted (182) from *The Golden Bough*, it is expressly said, however, that "unless the belief [in powers higher than man] leads to a corresponding practice, it is not a religion but merely a theology." The corresponding practice, again, is described as "an attempt to propitiate or please" the higher powers; and obviously such practices imply corresponding emotions—at least in their origin and in those who carry them out with sincere unction. Is it necessary to insist upon it?

There is a sort of *a priori* proof that the Australians are (or must be called) religious: "For the anthropologist, religion is a universal attribute of man, because his historico-scientific purposes require him to have a free hand in the following up of origins right back to the point where the direct evidence of history breaks off" (157). So, if you do not call totemism 'religion,' you are no anthropologist. But is there no beginning of anything in the history of culture; or may not that which Sir James Frazer calls 'religion' have originated at a later stage of culture than that of Australian totemism, not indeed *ex nihilo*, but by a synthesis of elements already existing, namely, a belief in spirits, funerary rites, and (no doubt) the sense of sacredness? At a controversy as to what should be called 'religion,' one soon feels tired; and it might be wise to disuse the word in Anthropology.

Recognising the profound influence of emotion on such observances as those involved in totemism, including the *Intichiuma* ceremonies, how shall the student convey to others the impression they have made upon himself? Shall he chiefly rely upon the describing of what happens, in the hope that the reader's sympathies will supply the rest; or shall he try to bring it home to the civilised bosom by using the language appropriate to what may be considered as parallel social phenomena amongst ourselves? The latter course (which is Mr Marett's) seems to imply that the state of mind amongst Australians during the *Intichiuma* ceremonies is much the same as that of cultivated Europeans during celebration of the Mass. In seeking to secure such ends as are socially approved, we are told, "the postulate of magic, in this sense, and of religion would seem to be the same; namely, that there is a power conferred by holiness of life which, through the mediation of holy men, can be made to abound for the common good" (206). Such a mode of expression (characteristic of the school of Durkheim) it would be very unjust to treat as a misrepresentation. Not false, it is unduly heightened by a generous desire to vindicate the savage against the inconsiderate undervaluation of obtuse observers. But less heightened it would be more effective. Probably there has been an unbroken evolution of the feelings involved in what we call the 'sacred' or 'holy' from the stage of Australian totemism to that of modern Christianity; but there has been an evolution; and those students seem to be justified who hold that such terms at the savage level may most fairly be rendered by our 'dangerous.'

Similarly, when it is suggested that a medicine-man's will 'for power' may be conceived as "a will to exert power over self, whereby a spiritual experience, indirectly involving a disposition towards a life of social service, may be attained for its own sake" (63)—what are we to say? This is an ideal, to which rare individuals may perhaps approximate. And thus we have indicated to us a good reason for not attempting to say much about the emotional side of savage institutions, namely, that the emotions are so variable, not only in the course of ages, but in individuals in the same age; nay, in the same man on different occasions of the same rite. What savages do may be faithfully described; what they think can be approximately translated for us by those who know their language; but what they feel we cannot be told; for two men may perform the same rite with very different feelings. And, indeed, to know what either of them feels may be difficult, or impossible, for the best observer. Many students must have tried to put themselves in the place of the *Naturmensch*. Is the result ever satisfactory? Does there not always remain a surd of impulse and emotion too obscure for words?

There are many other matters in this book which it would be profitable to discuss if space permitted, such as the judicious treatment of war and savagery in chapter II, and of the anthropological methods of Tylor, Gomme and Rivers in chapter IV. It is all earnestly thought and admirably written.

An Introduction to Sociology for Social Workers and General Readers. By Professor J. J. Findlay. London: Longmans, Green & Co. 1920. pp. xi + 304. 6*s.* net.

It is scarcely necessary to say that the reader will find in this book much that will stimulate and suggest, and that many will be induced to turn not only to the deepest sources of learning but to the world of experience with a clearer outlook and a firmer determination to get to the basic principles of social life. The book is divided into three sections dealing respectively with principles, types and organization. Of these the student will probably find the last two the more helpful; but the conclusions as to fundamental principles which are to be found in the first section should stimulate him to thought, despite the fact that he will probably wish that Professor Findlay had in some cases given a fuller exposition of the evidence.

It is somewhat startling at the present day, when many are convinced that we are suffering from the existence of too many committees, to be told that "we shall employ not less committees but more," that "every citizen must be at times inspector and manager" (p. 30), and that "the new principle of progress is concerned with numbers, with the size of the unit" (p. 30). One's thoughts turn to the view of Graham Wallas that members of committees are being reduced to the position of obedient shadows or of intriguers for small concessions from the holders of real power.

In the fundamental problem of the relation of the member to the group, the author's treatment illustrates, we believe, the transition from a naïve individualism towards a more adequate recognition of the social nature of man. Thus in the earlier pages of the first section we find such a passage as "uniformity of reaction to identical stimuli is the comprehensive factor which enables us to identify any individual as being a member of a group" and "to declare that the total is a social unit and not a fortuitous concourse" (p. 11). Later we find "This union is not an accretion of uniform particles attracted to one spot by identity of composition" (p. 103) and "they unite...because...they seek amid diversity to achieve a common purpose" (p. 109). No doubt these later quotations represent the view towards which the author is attracted, but the novice may be hindered in his progress towards it by the earlier passages we have quoted from the book, and he may require more guidance than Professor Findlay gives as to the ambiguity lurking in such a phrase as "common purpose."

Individual Differences in Output in the Cotton Industry. (Textile Series, No. 1.) By S. WYATT. Report No. 7 of the Industrial Fatigue Research Board. London: H.M. Stationery Office. 1920. pp. iii + 13. 6*d*. net.

This highly suggestive preliminary report shows that the various processes in the cotton trade can be classified according to the relative predominance of the human and mechanical factors and according to the consequent range of the variations of individual workers in output. The human factor proves to be of the greatest importance in weaving and winding and "it is in these processes, rather than in spinning, that efficiency may be increased and fatigue reduced by the elimination of waste in method and movement." The writer points out that the magnitude of individual differences in any single occupation may also depend on the amount of skill involved, and that a scientific basis for wage payments and duration of apprenticeship would result from a classification of processes according to the amount of skill involved and other conditions of employment.

The Psychology of Persuasion. By WILLIAM MACPHERSON. London: Methuen & Co. 1920. pp. 256. 6*s*. net.

This is a popularly written book, of no great psychological importance, interesting to the psychologist mainly for the illustrations with which it is enlivened, drawn from the fields of oratory, advertisement, salesmanship, cinematography, and art.

Mental Tests. By Dr PHILIP BOSWOOD BALLARD. London: Hodder and Stoughton. 1920. pp. ix + 235. 6*s*. net.

Dr Ballard has written this little book primarily for the teacher. Some of the chapters have already appeared in a slightly different form in the *Times* Educational Supplement and in the *Journal of Experimental Pedagogy*; two others are based on papers read before the Education Section of the British Psychological Society and the Educational Handwork Association. Chapter IV gives a useful translation, made by Mr Cyril Burt in consultation with Dr Simon, of Binet's well-known tests; chapters V and IX describe Mr Burt's graded series of reasoning and spelling tests. The book is very brightly written and will prove of great service to the teacher. Few are better qualified to write on the subject from the educational standpoint than Dr. Ballard.

An Introduction to the Study of Hypnotism: Experimental and Therapeutic. By Dr H. E. WINGFIELD. London: Baillière, Tindall and Cox. Second Edition. 1920. pp. vi + 195. 7*s*. 6*d*. net.

Dr Wingfield is one of the sanest, most expert and experienced of British hypnotists. The first edition of his book was exhausted within a short period after its publication many years ago. This new edition includes an account of the treatment of repressed memories by hypnotism; in other respects the book is substantially unchanged. For those who seek a reliable account of the methods of inducing hypnosis, of the phenomena of hypnotism and of treatment by hypnotism, no better book of its size can be recommended.

Quelques Traits de la Vie de Jésus: au Point de Vue Psychologique et Psychanalytique. Par Dr GEORGES BERGUER. Genève et Paris: Atar. 1920. pp. cviii + 267. Fr. 15.

This is a series of lectures delivered at the University of Geneva in 1917 and 1919. "There is a supreme interest," says the author, "in returning to the primitive documents we possess concerning Christ, and in bringing to bear on this grand Figure all the light of the modern sciences. Research in this field must evidently be accompanied with considerable prudence and with profound humility." The last sentence gives the keynote to the book. From the psychological point of view its results are extremely disappointing. The author's psychoanalytic efforts are feeble, being based on inadequate qualification for the task. His comparisons of Christianity with other mystic religions are superficial. Fear of the *odium theologicum* seems to inhibit the author from start to finish.

Beauty and the Beast: an Essay in Evolutionary Aesthetic. By S. A. MCDOWALL. Cambridge: University Press. 1920. pp. 93. 7*s.* 6*d.* net.

The basis of this book is Croce's theory of the Aesthetic. From Croce, however, the author dissents notably in the former's "rejection of a metaphysic and his denial of a God." In the Introduction he informs us how in attempting to form a philosophy of religion, he had "tried to find in Love, which is the very nature of Godhead, an essential impulse towards creation," how there he found that "something vital had clearly been left out" and finally that "in Beauty I seemed to find what I had missed." The essay is interesting but contains little of psychological importance.

Lectures on Industrial Administration. Edited by B. MUSCIO. London: Sir Isaac Pitman & Sons. 1920. pp. viii + 276. 6*s.* net.

In July 1919, a 'school' for the study of certain problems connected with industrial management was held in Cambridge. A selection of the lectures there delivered, by Professors Sorley, Pear, Cullis and Collis, Messrs Bowie, Wrong, Stelling, Burt, Wyatt, Myers, Fleming and Sargant Florence, is published in the present volume. The school was so largely attended and it attracted so much interest that the publication of these lectures is likely to satisfy a widely felt need.

Mental Fatigue. By Dr G. E. PHILLIPS. No. 40 of the Records of the Education Society, Teachers' College. Sydney: W. A. Gullick. 1920. pp. 103. 7*s.* 6*d.*

This is a thesis prepared, under the direction of Prof. Spearman, at University College, London, for the London University doctorate in Science. It contains an interesting historical survey of recent work on fatigue and a detailed account of the writer's own experiments which deal with the effects of different lengths of rest pauses, with the transfer of fatigue from one kind of mental work to another, and with the form of the work curve during very brief periods of work. The concluding chapter is devoted to a theory of fatigue based on Lehmann's (here consistently spelt 'Lehman's') application of certain chemical laws to muscular output. The author deduces from his experiments that "fatigue is a general phenomenon, and is not specific to the task being performed," that although it is produced 'generally' it is transferred 'specifically,' and that in work periods of 1 or 2 minutes the work curve has a constant shape "independent alike of the task and of the subject who does the task."

Human Efficiency and Levels of Intelligence. By H. H. GODDARD. Princeton: University Press. 1920. pp. vii + 128. 6*s.* 6*d.* net.

The author publishes a course of four lectures delivered in Princeton University during April 1919. His contention is "that the number of people of relatively low intelligence is vastly greater than is generally appreciated and that this mass of low level intelligence is an enormous menace to democracy unless it is recognised and properly treated." He fears the rapid increase of inefficiency and delinquency "through the natural propagation of hereditary feeble-mindedness," he urges the segregation of the feeble-minded from childhood onwards, and recommends the evaluation of the degree of intelligence needed for different occupations, to which only those should be admitted who have the necessary intelligence.

Lehrbuch der Experimentellen Psychologie. Von Prof. J. FRÖBES S.J. Zweiter (Schluss-) Band. Freiberg im Bresgau: Herder & Co. 1920. pp. xx + 704. £1. 6*s.*: geb. £1. 10*s.*

This book maintains all the faults and virtues of its companion volume. It deals with speech, attention, memory, intelligence, emotion, will, personality, sleep, hypnosis, mental disorders, etc. A wonderful scrap book of opinions, culled from the writings of hundreds of investigators, good, bad and indifferent, it is a monumental work of Teutonic diligence, but wholly devoid of *Geist*.

Intelligence and Social Valuation: a practical method for the diagnosis of mental deficiency and other forms of social inefficiency. By Dr R. A. BERRY and S. D. PORTEUS. New Jersey: Vineland Training School. 1920. pp. 100. $1.50 post paid.

The graded series of Porteus maze-tests, it is claimed, "represent an attempt to evaluate socially valuable characteristics not fully tested by the Binet...." The authors propose that "cases should be regarded as 'potential social inefficients' if the average of the Binet-Porteus quotients fall below 75." They investigate the relation of brain capacity to mental development, and discuss the correlation of various physical and mental measurements. The Report includes a series of tables based on a large number of measurements which cover a very wide field of research.

The Mystery of Life as Interpreted by Science. By R. D. TAYLOR. London and Felling-on-Tyne: Walter Scott Publishing Co. 1919. pp. 176. 3*s.* 6*d.*

A National System of Education: some recommendations for establishing it in England during the decade ending ten years hence. Published for the Federal Council of Lancashire and Cheshire Teachers' Associations: Manchester University Press. London: Longmans, Green & Co. 1920. pp. 78. 1*s.* net.

La Philosophie d'E. Boutroux. Par Dr. A.-P. LA FONTAINE. Paris: J. Vrin. 1920. pp. iii + 89. Fr. 3.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

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July 10, 1920. Complex and Sentiment, by W. H. R. Rivers.

Habit Formation in Guinea Pigs, by E. M. Smith.

Demonstrations of (*a*) Apparatus for testing Psycho-galvanic Reaction, by E. Prideaux; (*b*) Some New Tests of Auditory Sensation, by H. Hartridge; (*c*) A Method of experimenting on the Relation of Affection and Memory, by W. Whately Smith; (*d*) Some Printing Trade Tests, by B. Muscio.

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(*b*) Industrial Section.

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DISORDERS OF SYMBOLIC THINKING AND EXPRESSION[1]

BY HENRY HEAD.

1. *Introduction* (pp. 179–181).
2. *These disorders of speech are not due to destruction of 'images'* (pp. 181–184).
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4. *Conclusions* (p. 193).

1. INTRODUCTION.

THE power to make use of words was acquired at a late stage in the evolutionary history of man. Before he learnt to formulate his thoughts in speech, he was capable of a wide range of accurate discrimination, which could express itself in action only. Progressive development of the higher cerebral centres led to increased discriminative powers; and speech, one of the highest and most recently acquired functions, was from the first concerned with expressing relations.

In animals, cries uttered during states of pleasure, fear or anger, are capable of exciting similar passions in hearers of the same species, and specific sounds may form an essential link in what Pavlov has called 'conditioned reflexes.' 'Clucking' is as effective a call to the chickens as rattling the manger to a horse; both have become definite indications of the presence of food.

Man enormously widened this association between certain sounds and consequent actions, and was finally able to express the relation of some external object to himself in the form of a proposition. He not only called the white substance, which covered the ground in winter, "snow," but formulated the sensory impression it evoked in the phrase "snow is cold." In this way he became capable of manipulating words in thought. Not only could he express the relation of external objects to himself and register his impressions, but he was able to convey them to others.

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[1] Presented at the Oxford Philosophical Conference, 25 September, 1920. For a description of the tests and many of the clinical facts on which the conclusions expressed in this paper are based, see the writer's "Aphasia and Kindred Disorders of Speech," *Brain*, 1920, XLIII, 87–165.

But there always remained elements in thought which were not associated with words. Speech is a discriminative movement capable of fine degrees of adjustment; it is essentially an intellectual mechanism. The more nearly the mental state approaches pure feeling, the less easily can it be expressed in words alone, apart from gesture and the tone of the voice. Even in the gravest cases of aphasia the patient is evidently fully aware of his emotions, and can express them clearly in gesture and action. Under the influence of emotion he may be able to say 'yes' and 'no,' and even phrases such as 'Oh! dear me,' 'I know it, I can't tell,' though he is entirely unable to evoke the words voluntarily or to command. In order that words can subserve intellectual activity, they must be mobile and capable of manipulation at will.

At first sight it might seem difficult to understand how gross destruction of the brain could produce loss of so complicated an activity as speech, without reducing the patient to a state of dementia. But we are familiar with the high-grade loss of sensation due to injuries of the cerebral cortex; the qualitative and affective aspects are not materially diminished, whilst the power of estimating the spacial relations, graduated intensity and similarity or difference of external stimuli may be gravely disturbed. All these changes may occur without materially reducing the patient's intellectual capacity, apart from his power of appreciating sensory relations. Speech is a more complex psychical aptitude; but it can be disturbed without of necessity producing grave intellectual defect, apart from the loss of those functions associated with the activities which underlie the perfect use of language.

The fact that speech can be disordered by injury to the brain is one of the most wonderful means placed in our hands for investigating the relation of mind and body. But, unfortunately, all the elaborate and careful work expended on this fascinating problem has been vitiated by the conception that speech was a well-defined intellectual function, which was strictly localised in some particular site in the brain. Attention was concentrated on correlating the site and extent of the anatomical destruction with the character of the disorder of speech. Such efforts were bound to fail, because the middle or physiological term of the problem was neglected. The utmost skill and care expended on determining the nature and extent of the anatomical lesion will not reveal, except in the roughest manner, the form assumed by the loss of speech during life. Within certain limits the severity of the lesion and the rapidity of its onset are more important than its anatomical extent. A small limited destruction of substance arising acutely may produce a more profound and widespread loss of function than one of wider extent but gradual incidence. A lesion within an area on the surface of the brain, associated with the mechanism of speech, may produce in one case 'total aphasia,' in another 'partial aphasia' ('motor' or 'sensory'), and, in a third instance, be accompanied by no obvious disorder of language. At the root of most of the work on 'aphasia' lies the fundamental error of ignoring the physiological changes intervening between the anatomical lesion and the psychical states with which it is associated.

An even more disastrous fallacy consists in attempting to correlate anatomical changes categorically with metaphysical conceptions of the forms assumed by psychical activity. Destruction of the substance of the brain can only disturb the act of speech, because it interferes with those physiological processes necessary for its perfect execution. Now, there is no reason to suppose that any groups of cerebral functions correspond to the 'memory of words,' 'reading' or 'writing,' or even to 'speech' itself. Any lesion, which gravely disturbs 'speech,' affects activities which are not usually classed with the use of language; and the more carefully the patient is examined, the less certainly does his disorder correspond to any preconceived category.

2. THESE DISORDERS OF SPEECH ARE NOT DUE TO DESTRUCTION OF 'IMAGES.'

As soon as it was evident that speech could be affected by an injury to the brain, observers began to seek for some mechanical explanation of this indisputable fact. Gall had already given the frontal lobes as the seat of memory, which he divided into six different varieties; there was the memory of things, the memory of locality, name-memory, verbal and grammatical memory, and that for numbers. It was an easy step to suppose that such 'memory' was based upon 'images' which were 'stored up' in the cortex. These were destroyed by the organic injury, and speech suffered from a want of specific memories. Bastian, in 1869, started from the *a priori* assumption that we think in words, and that words are revived in the cerebral hemispheres as remembered sounds. He was the first to introduce the conception of 'word-deafness' as the cause of defects of language, a theory subsequently developed more fully by Wernicke. Under the influence of Munk's extirpation experiments, 'mind blindness' crept into the work on aphasia. Finally, a general idea grew up that most of these high grade disorders of speech could be explained by loss of auditory and visual images; the remainder were due to defective motor activity.

The existence of speech in persons devoid of auditory or visual imagery, and the wide diversity in the nature of the normal power to form images, would render any such theory of the mechanism of speech improbable. Images play a part in the use of words and numbers, but the extent to which they are used varies greatly; they are not a necessary and fundamental factor in speech.

Moreover, if the lesion remains strictly unilateral, the phenomena are not those which would accompany 'mind blindness' or 'mind deafness.' For a patient who suffers from 'mind blindness' is unable to recognise the significance of an object, so long as he employs sight alone, although he can do so if allowed to use his other senses. In the same way, 'mind deafness' postulates inability to appreciate sounds, and particularly spoken words, with complete comprehension of the same words presented in print. In uncomplicated cases of aphasia, no such specific difference occurs; the loss of function affects words or their meaning, whether they are heard or seen. On the other hand, with certain bilateral lesions of the brain, images may suffer, either alone or in association with true aphasic manifestations.

The forms of imagery usually found amongst an English-born population is mainly visual, and the majority of my patients possessed strong powers of visual memory. I therefore included the following tests in my systematic examination. The patient drew some object from a model in front of him, and after its removal was told to reproduce it from memory. This is no necessary evidence of visual imagery; but as he drew from memory, he was asked to describe the colour, shape and pattern as far as possible. He then attempted to draw a plan of some room with which he was familiar, such as his ward in the hospital. If this presented any difficulty, I marked on a sheet of paper an oblong to represent the bed, and asked him to indicate on the paper the situation of the surrounding objects. As a further test he was asked to describe the way along some familiar route either within or outside the hospital.

Throughout these tests most of my patients laid stress on the fulness of their inner vision; the difficulty lay in formulating their conceptions. No. 5, when examined in another part of the hospital, drew a plan of the beds in his ward incredibly badly; walls, windows, and even the beds themselves were wrongly placed. But he insisted, "I can see all in my mind but I can't put it down; I've got it all in a picture in front of me," waving his hand before his eyes. He described accurately the colour of the curtains and the red covers of the beds; nor had he the least difficulty in recalling the uniform, both of nurses and sisters. He could indicate the position of the objects round his bed by pointing, but could not transfer their relative situation to a plan. Another patient with severe aphasia was shown a jug, and after its removal drew the outline correctly from memory, and added that it was covered with red and green patterns.

No. 8, who had a considerable domestic reputation as a draftsman before he was wounded, tried to draw a landscape from memory. He explained his complete failure as follows, "I want the thing in front of me. I don't know how to build it up. If I had the view in front of me, I could make you a decent sketch. I can see it in my mind but I can't put it down."

Another intelligent patient, who could not comprehend with certainty the meaning of words or figures, gave me the following example of her difficulties. "People ask me what 'bus are you going by, and I don't know. A number conveys nothing to me. But when I see a 'bus with Barking on it, I see in my mind Barking Park, and then I know that's where I want to go."

One of the ablest of my young officers tried to tell me how he would go from the hospital in which he was lying to the War Office. "Suppose I was going from the Army and Navy, that's what it's called, just round here (Army and Navy Stores), I should then say some place to a hospital about a quarter of a mile away. I remember the hospital is on the left on the way to the War Office, about half-way. I believe it's the Abbey. No it's near the Abbey, on the way to the War Office." Later he added, "I saw the hospital." I put as a question "The Abbey?" and he replied as follows. "It's here and then it's gone again, and I have to feel for it again. The only thing I can remember is the opening, the big opening where everybody goes in. I can get that" (moving his hand in the form of a large arch). Since it was obvious that he had described Westminster Hospital and Westminster Abbey, I asked "Have the Hospital and Abbey anything to do with one another?" He answered "Nothing except my focus, the place of them, the distance, that is all. You see, it's like this; with me it's all in bits. I have to jump like this," marking a thick line between two points with a pencil. "Like a man who jumps from one thing to the next. I can see them but I can't express. Really it is that I haven't enough names."

He repeatedly asserted that he could 'see,' but could not express what he saw. This was exemplified when he was asked to read to himself the following account of a famous prize fight, a few days after its occurrence. "Carpentier, the French champion, met Beckett, heavy-weight champion of England on Thursday, December 4th, at the Holborn Stadium. Beckett was knocked out in 74 seconds by a right hand hook to the jaw." At the end of six minutes he returned the paper and started to tell me what he had read in the following words. "Carpentier met Beckett in the Holborn—say—so—Sadium, I used to know it, and Carpentier being the champion now—who is—oh no—who met Beckett—now the champion in England—knocked—er—knocked—er—Beckett out in 74 seconds—with a right—with a right hook to the jaw." He then added "I wasn't thinking so much of the fight—but what I had been reading—I saw the picture of 74—seconds—saw what I had read."

I then said "Can you see the fight?" and an extraordinary change came over his narration. "Now I think about the fight I see the picture of Carpentier. I see Beckett falling—I see the Ref—Ref—what you call it—Referee—. The last thing I can picture is Carpentier standing like this with both hands like this." He sprang from his bed and assumed an attitude with both hands on his hips looking down towards the ground, in the position of Carpentier in the photographs of the termination of the fight. The change in manner from the puzzled aspect of a man slowly recalling what he had read to the brisk, vivid, smiling person describing what he saw was remarkable.

Auditory images, stored up in certain areas of the cortex, were supposed to be responsible for the 'memory of words,' and normal speech necessitated the revival of these sounds in the mind; aphasia followed as a natural consequence, when these images were destroyed by an organic lesion. But this hypothesis is not sufficient to account for the clinical facts. For a patient may be unable to recall the names of a series of colours, though he can describe them in metaphorical terms; here it is not 'auditory images' of words that are lacking, but power to find the appropriate name. On the other hand, he may be able without difficulty to select a printed card bearing the name of a colour shown to him, although he can scarcely utter or write a single word; it is the capacity to use words and not the power of naming that is affected in this case. The fact that verbal and nominal disorders may occur independently of one another shows that the hypothesis of 'auditory images' is inadequate to explain the phenomena. So long as the lesion is confined to one half of the brain, there is no reason to suppose that loss of 'images,' visual or auditory, is primarily responsible for these defects of speech.

3. THE FUNCTIONS WHICH ARE DISTURBED IN APHASIA AND KINDRED DISORDERS.

From 1868 onwards Jackson insisted that the chief mental activity disturbed in these disorders of speech was the use of words in propositions; those which remain are the same words used 'non-propositionally.' This is profoundly true, and, had we appreciated the wide-reaching significance of this generalisation, we should have been spared many years of wandering in the wilderness.

Although this conception covers the larger number of the facts I have observed, there are some aspects of the loss of function in aphasia which are not strictly comprised under the heading of 'propositionising.' Nor do I believe that it is possible to discover any exact and comprehensive definition which includes all these disordered processes. I shall therefore attempt to enumerate the various activities which may suffer in consequence of a unilateral lesion of the brain, and, for this purpose, it is necessary to consider more particularly the slighter manifestations; severe loss of speech may render all detailed examination impossible.

Words, numbers, pictures and every act which depends upon the use of these symbols in constructive thought may be affected. Any mental process is liable to suffer, which demands for its performance exact comprehension, voluntary recall, and perfect expression of symbolic representations.

I have therefore suggested that the various functions disturbed in aphasia and allied conditions might be spoken of as 'symbolic thinking and expression,' because it is mainly the use of words, numbers and pictures which suffers in these disordered states. But this name must not be taken as in any way defining the group of processes affected; I should have preferred, had it been possible, to have employed some entirely indifferent term. For it is symbols used in a particular manner which suffer in these disorders, and not all symbolic representations.

This group of functions is not uniformly disturbed in every instance; cases of aphasia differ profoundly in their clinical manifestations. The majority, especially in the earlier stages, show evidence of widespread defects in the use of language; the more acute and severe the lesion, the graver and more extensive is the disorder it produces. Many of these changes may disappear, and sometimes the patient recovers entirely; but, in certain cases, one or more aspects of symbolic thinking and expression remain affected over a considerable period, and the aphasic manifestations consequently assume some particular form.

Analysis of these clinical varieties seems to show that the various functions included under the term 'symbolic thinking and expression' can be dissociated in different ways under the influence of organic injury. They do not correspond to different stages of dissolution or recovery. Nor do they reveal directly the elements out of which language is built up. On the contrary, they show the components into which a highly complex set of psychical processes can be separated by destruction of certain portions of the brain. Provided we bear these principles in mind, we are justified in grouping the clinical manifestations under separate headings. Fortunately, all the functions included under symbolic thinking and expression are rarely lost in the same patient; for, if this were the case, he would be reduced to a state in which all detailed examination was impossible.

To each of these groups I have given a name chosen from the most salient defect in the use of words; these names are purely indicative, and are not intended in any way as defining a specific psychical function.

(*a*) *Verbal Defects.* In severe forms of this disorder, the patient's utterance may be reduced to 'Yes' and 'No,' and even these words cannot always be evoked for voluntary use. As speech returns, his vocabulary increases, but his enunciation is slow and halting. Any word he is able to recall can, however, be used for naming an object. It may be so badly pronounced as to be scarcely recognisable, but it is applied correctly. When the patient attempts to repeat what has been said to him, the articulatory sounds are imperfect, but he can usually utter more words than are possible spontaneously. It is characteristic of this form of aphasia that words are evoked with difficulty, and tend to be abnormal in structure.

After the stage of neural shock has passed away, the power of choosing an object to oral or printed commands becomes perfect, and even orders necessitating choice can be carried out correctly, if given in print or in words spoken aloud.

As the spoken vocabulary increases, the power of writing is regained, although throughout it tends to show the same errors as articulatory speech. These patients cannot spell and find difficulty in remembering the order of the letters, even in simple words. They write more easily to dictation, but are unable to carry in the memory a string of words or a long phrase.

The power of reading to themselves with enjoyment is spoilt by difficulty in remembering a series of words accurately; they are frequently compelled to look back to the beginning of a long sentence in order to obtain its full meaning.

The verbal aspect of numerals is affected, but not their significance. Thus, when looking up the page of a book or scoring at cards, the patient may utter the wrong number, but acts as if he had said the right one. Simple arithmetical operations can be carried out correctly, except in severe cases; then it is not the process of addition and subtraction which is forgotten, but the act fails, because of the difficulty in remembering the requisite figures.

These patients can draw, play card games and enjoy jokes set out in print or in pictures. In fact, the disorder from which they suffer affects mainly verbal structure and words as integral parts of a phrase; their nominal value and significance are perfect.

(*b*) *Syntactical Defects.* This is an easy form to distinguish, because the patient tends to talk jargon. Not only is articulation of the words ill-balanced, but the rhythm of the phrase is defective, and there is want of grammatical coherence. The power of naming objects may be retained in spite of the jargon by which he is hampered. Not infrequently, when he cannot utter a word or when the sound emitted is incomprehensible to his auditor, he writes the name correctly.

But although he can write single words, any attempt to convey a formulated statement is liable to end in confusion. Patients suffering from the more severe degrees of this affection cannot write a letter, but, in the slighter cases, writing is easier than articulatory speech. They can all copy correctly and transcribe print into cursive handwriting.

These patients can understand what they read to themselves, provided they are not compelled to reproduce the meaning in words; for then internal speech is also disturbed by jargon.

This disorder is essentially one of balance and rhythm in symbolic expression, and syntax suffers greatly. The patient has plenty of words, but their production is ataxic; this leads him to talk jargon and renders difficult even internal formulation of their meaning.

(*c*) *Nominal Defects.* This is essentially a loss of power to use names and want of comprehension of the nominal value or meaning of words and other symbols. Not only does the patient fail to name objects placed in front of him, but when asked to point to one of them named aloud or in print, the choice, even if correct, is made slowly and with effort.

Such patients read with extreme difficulty, especially if they attempt to spell out the words. Writing is gravely affected, and, although they can copy printed matter correctly in capitals, they may be unable to do so in cursive handwriting. Writing to dictation together with all actions demanding choice are carried out imperfectly to spoken commands; but direct *viva voce* repetition is not affected.

They can usually count and say the alphabet, but suffer from defective appreciation of the meaning of single numbers or letters. This interferes with the power to carry out simple arithmetical operations, and appreciation of the relative value of money is usually more or less affected. Games, such as cards, which demand rapid and correct recognition of names and power to register a score, are impossible, though chess, draughts and dominoes may be played correctly.

Drawing from a model, or from memory after the object is removed from sight, is easily performed; but, when the patient is asked to draw from imagination some such figure as that of an elephant, the result is extremely unsatisfactory. All the distinctive parts are usually omitted.

One of the most instructive forms, assumed by the loss of function, is the difficulty experienced by the patient in drawing a ground plan of some room with which he is familiar. However badly the verbal aphasic may draw, he usually succeeds in indicating the relative position of the windows, doors and principal pieces of furniture. But the patient with nominal defects, fails to produce a correct plan, and tends to slip into an attempt to express the constituent parts in elevation; and yet when asked "where is the table?" or "where is the window?" he can point to their situation correctly.

(*d*) *Semantic Defects*. These consist in a want of recognition of the full significance of words and phrases apart from their verbal meaning. Other functions suffer that have nothing to do with verbalisation, for in this form of the disorder there is loss of meaning in thought. The patient may understand a word or short phrase, and can appreciate the various details of a picture; but the ultimate meaning escapes him.

Thus, although he understands the significance of the words 'Summer' and 'Time,' and knows that 'Summer-time' is "something to do with the Daylight Saving Act," he is unable to say whether the clocks are put forward or back when it begins.

He fails to comprehend the final aim or goal of an action imposed upon him from without. He cannot formulate accurately, either to himself or to others, a general conception of what he has been told, has read to himself, or has seen in a picture, although he can enumerate many of the details of which it is composed; the knowledge he has gained is episodic. Such patients can read and write, but the result tends to be inaccurate and confused.

Counting is possible, and the value of numerals and of coins may be recognised; but arithmetical operations are affected from want of appreciation of the nature of the procedure required.

Such patients fail to understand jokes, which demand complete comprehension of printed words or pictures. They cannot play card games or put together puzzles which confuse them greatly.

These semantic disorders interfere seriously with the activities of daily life and render the patient useless for any but the simplest employment; and yet his memory and intelligence may remain on a comparatively high general level. He does not forget people or places; he can recall spontaneously events both recent and remote, and may be able to give valuable information with regard to his disabilities. But, if he is set the task of retailing accurately some story which he has been told or has read to himself, he is liable to become confused and to omit many factors of importance.

Amongst the many functions comprised under the term 'symbolic thinking and expression' some are affected in one case, some in another, and the clinical manifestations of aphasia depend mainly on the incidence of this loss.

But, apart from this character, the degree to which these functions are disturbed has a profound effect upon the condition of each individual patient. One of them may fail to carry out a test correctly, which another can execute without difficulty; and yet both seem to belong to the same functional group. Moreover, as the aphasia gradually passes away, the patient may respond so differently to the various tasks he is asked to perform, that at first sight the nature of his disability seems to have changed. Thus, not only the form but also the degree to which symbolic thinking and expression are affected has a profound effect on the nature of the clinical manifestations.

The more nearly a symbolic action approximates to a frank proposition, the greater difficulty does it present, and the patient will probably fail to execute it correctly. Conversely, the more closely it corresponds to matching two sensory patterns, the less likely is it to be disturbed. Highly complex symbolic acts suffer more gravely than those of lower propositional value.

For example, if a patient seated opposite the observer attempts to imitate a series of movements, which consist in touching an eye or an ear with one or other hand, he may fail grossly. He can however execute them perfectly when the observer stands behind him, and they are reflected in a mirror. But, when he is asked to write down what he sees, he again fails as badly as before.

For in order to imitate movements sitting face to face, he must formulate the various factors, which make up the command; whilst to copy them when reflected in a mirror is an act of almost pure imitation. As soon, however, as he is asked to write down what he sees in the glass, he is compelled to formulate the task in words and fails to describe gestures he can imitate perfectly.

The higher the propositional value of the mental act, the greater difficulty will it present. Thus, a patient may be able to execute a printed command to hold up his hand, although he is unable to carry out an order to touch with it his eye or his ear. The addition of the second factor has rendered the task too difficult. The larger the number of possible alternatives presented by the order, the more certainly will the desired action be defective. It is possible in this way by suitable tests to grade the patient's disability. First, he is required to carry out a series of movements, such as 'lift your hand,' 'shut your eyes,' etc., which name one part of the body only. Then the right or left hand is specified, and the task is gradually increased in severity until finally it is brought up to the multiple alternatives of the full hand, eye and ear tests. Somewhere on this ascending scale of difficulty, most aphasics will break down.

All propositions express an abstract relation; but even the same generalised statement may vary in difficulty according to the means by which it is expressed. For example, during the compass test, the aphasic patient may be entirely unable to record in speech or in writing whether he has been touched with one or two points. But, if the figures 1 and 2 are written on a sheet of paper, he can indicate correctly whether the contact was single or double. Under the ordinary conditions of this test, he is not only compelled to formulate to himself his sensory impressions, but he must express them in verbal symbols; when it is carried out according to the second method, he has only to determine whether his sensations are of one or of two points, and the means of expression lie on the table before him.

Although the patient is conscious of the position in space of objects around him, he may be unable to express their relations. Thus, when asked to draw a plan of some room with which he is familiar, he may fail completely. But, if a figure is drawn to represent, for instance, his bed in the hospital ward, he may be able to indicate the relative position of the various surrounding objects; this is a lower method of symbolic expression. There are however cases in which even this is impossible; the patient can no longer formulate the relation of the doors, windows or beds to one another, although he can still point to the situation of any one of them. He can remember the place of the window or the door, but he cannot express the relative position they bear to one another.

When the defect consists mainly in want of power to evoke words or names, the patient may be unable to name a set of common objects, although he can choose them correctly to oral or printed command. The verbal symbol conveys its proper meaning, when presented ready formed in sound or print; but to produce it freely at will demands a greater perfection of symbolic thinking and expression.

In the same way when an aphasic calls the scissors "That's the tweezers to cut with," or speaks of a kitten as "a little fur-child," he is expressing a lower grade of symbolic recognition. Patient no. 2 was unable to name a series of colours placed before him and could not choose them correctly to oral or printed commands. But if, instead of the names, he was allowed to call black "What you do for the dead," red "What the Staff wear" with similar descriptive phrases for each of the other colours, he named them all correctly. These metaphorical modes of expression are simpler than direct uni-verbal nomenclature; for the more concrete is lower in the hierarchy of mental processes than the general and abstract.

Behind every propositional expression lies recognition of the meaning of symbols. I must be able to appreciate the full-significance of words, numbers and pictures, before I can pose them in propositional form. This loss of meaning is the cause of many aphasic defects. Thus, the short and long hands of the clock have acquired a significance which converts each one of them into a direct symbol, and they are often confused or used wrongly in cases of aphasia. Moreover, we are in the habit of dividing the space between any two numbers on the face of the clock into portions of an hour, giving to each its symbolic value. In many forms of disordered speech, the patient no longer sets the hour hand at a point proportionate to the position of the minute hand. Told to set a quarter to six, he places the short hand at six and the long hand at nine, so that it is impossible to discover without questioning him whether he intended to set 6.45 or 5.45. Not only the hands of the clock but their relative position on the face have acquired a meaning which is disturbed in aphasia.

When adding two numbers together, it is easier to reach the answer by counting, or progressive movement, than by a direct proposition. Thus, many aphasics can add 6 and 3, by saying to themselves 6, 7, 8, 9, but cannot formulate $6 + 3 = 9$. For the same reason some of them could set the clock correctly to a command in railway time, but not if it was expressed in words. Told to set the clock at 4.45, the patient placed the hour hand at 4 and then swept the other one round the face to a position he associated with 45 minutes. But asked to set 'a quarter to five' he placed the one hand at 5, whilst the other hovered uncertainly between 'a quarter past' and 'a quarter to.' On the other hand, when he attempted to tell the time set for him on the clock, it was equally difficult to evoke the names '4.45' and 'a quarter to five.'

Many of the disorders I have described are the result of inability to recognise the nominal or verbal significance of symbols. If, however, the patient fails to appreciate with certainty their ultimate meaning in thought, he is liable to become confused over the intention of an action, whether self-suggested or imposed from without. He no longer perceives clearly the goal at which he must aim. Thus, a young officer was unable to put together his belt when the slides had been displaced, or collect the materials he required before beginning to shave. Another patient could thread the quadrilateral frames for his bee-hives, if the action consisted in bringing the wire across from one side to the other and back again through neighbouring holes; but as soon as he attempted to go from corner to corner he failed entirely.

Perfect symbolic thinking and expression demand that words, numbers, pictures, and all they stand for in thought, should be mobile and susceptible of perfect voluntary manipulation. It is this aspect that is mainly affected in the verbal and syntactical forms of aphasia; they are not due to a dysarthria, but are produced by defective conceptions of the structure and rhythmic balance of the symbol, which interferes not only with articulatory speech, but also with internal verbalisation. One of my verbal aphasics, when saying the alphabet aloud, stumbled over R; he explained that he wanted to say A because he was thinking of 'are.'

Thus, in verbal and syntactical aphasia, it is the idea of the structure of the word which principally suffers both in internal and external speech. With such disorders, uttered words are more affected than the verbal processes of thought, and it is for this reason that I have spoken of symbolic expression. On the other hand, nominal and semantic defects mainly disturb the use of words in the varied processes of thought and their perfect employment as language symbols.

4. CONCLUSIONS.

The points I have tried to bring forward may be summarised as follows:

(i) A unilateral lesion of the brain, affecting the use of language, disturbs a number of psychical processes, which cannot be grouped under such headings as 'speech,' 'reading,' 'writing,' 'motor' or 'sensory' activities, or any other *a priori* categories.

(ii) I have ventured to apply to them the term 'symbolic thinking and expression,' because they consist mainly of the use of symbols in language and in thought; but it is symbols used in a particular manner which are affected, and not all symbolic representations. This term is in no sense a definition, for this group of functions cannot be comprised within any single general conception; they can be determined solely by experimental observation.

(iii) The various manifestations of aphasia and allied disorders of speech cannot be explained by destruction of sensory images. These may remain intact, although they cannot be used voluntarily as part of the symbolic mechanism of language.

(iv) These researches show that the two aspects of meaning inherent in the use of symbols may be separated by suitable lesions of the brain. Want of perfect recognition of verbal significance leads to a defective power of naming; whilst what I have called 'semantic defects' produce a loss of the general meaning of words, phrases, numbers and pictures.

THE OUTLOOK FOR VOCATIONAL PSYCHOLOGY[1]

By FRANK WATTS.

From the Laboratories of the University and Municipal College of Technology (Department of Industrial Administration), Manchester.

I. *The two lines of advance already followed in vocational psychology* (pp. 194–197).
  (a) *Success has been achieved in the placement of routine workers.*
  (b) *Can we hope for success in diagnosing higher qualities of mind?*
II. *The relation of general ability* (*g*) *to the specific intellectual aptitudes* (pp. 197–200).
  (a) *So far this relation has not been clearly understood.*
  (b) *Imitative tests must be used until we can psychograph the vocations accurately.*
  (c) *Some defects of the 'empiric' and 'analytic' tests.*
III. *An attempt at an analysis of human ability* (pp. 200–205).
  (a) *The 'g,' 'w' and 'c' factors in intelligence.*
  (b) *The need in our tests for appeal to the complete intelligence, not merely to the intellectual qualities.*
  (c) *Some suggested methods of testing the higher human qualities.*

I.

THE value of psychological tests for the determination of vocational fitness is now generally recognised to be considerable. A steady succession of investigators has demonstrated their wide possibilities since S. B. Thompson, by engaging as bicycle-ball inspectors only those girls whose discriminative reaction times were speedy, showed that a careful selection of employees for specific tasks might enormously increase industrial efficiency. The success of Walter Dill Scott in choosing salesmen by psychological tests, of Hugo Münsterberg in estimating the abilities of street-car drivers by their reactions to an experimental situation which produced as nearly as possible the mental conditions of their work, of E. L. Thorndike in diagnosing capacity among workers in the clerical and academic professions, of Henry C. Link in differentiating among good, bad and indifferent applicants for routine work in the engineering industry, and of the American psychologists as a whole in selecting on a huge scale the men most suitable for executive positions in the United States army during the recent world-war, provides indisputable evidence of the vitality of vocational psychology in America.

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[1] A paper read at a meeting of the British Association (Psychology Sub-section), Cardiff, 27 August, 1920.

It may, indeed, seem at first sight to some that the field of experiment already covered embraces in its broad sweep little less than the whole province of industry, but there is still much untrodden and difficult ground to explore before we shall be able to say that complete success has been achieved. Moreover, there are certain efficiency experts of unquestionable shrewdness who declare that by quite other means and without the elaborate methods of statistical science they have gained a point of vantage from which they can clearly discern the nature of the more elusive character qualities which we have found it extremely hard to track down and study at close quarters; indeed, they dispute our ability either to dispossess them or to move nearer by laboratory methods to the solution of those problems which have hitherto baffled us. So difficult is it to see the whole prospect in sober perspective, uninfluenced either by ill-founded confidence or unwarranted despair for the future of applied psychology, that an exacter analysis of how things stand is desirable.

In this connexion it may be profitable in the first place to distinguish between two streams of experimental activity which issued originally from different conceptions of the problems to be solved. On the one hand there has been the more recent search for authentic tests for general intelligence with which the name of Alfred Binet will always be predominantly associated, while on the other hand, we have witnessed the attempt initiated by F. W. Taylor, and taken up by our industrial organizers, to select workers for specific occupations not so much in virtue of their general all-round ability as for those particular aptitudes (such as steadiness of hand, accuracy of movement, rapidity of reaction, or quick recognition of sizes and shapes, and so on) which would facilitate their economical and effective adaptation to the demands of an industrial system, based on the functioning of highly specialised activities.

For much of the routine work to be done to-day scientific management, as we know it in the form popularised by Taylor, does not need the complete man endowed with wide powers of intelligence; it calls merely for animated abilities of a minutely differentiated kind. Thus, for example, Taylor wrote with respect to his ideal pig-iron loader, "One of the first requirements of a man who is fit to handle pig-iron as a regular occupation is that he more nearly resembles in his make-up the ox than any other type[1]."

In those industries in which Taylor's dream is becoming realised of an organization which involves a distinct separation between the planning department and the performing department, so that all the brain-work falls to the former, and to the latter all the bodily toil, it has not been found difficult to devise tests for the discovery of workers possessing specific aptitudes utilisable in tasks of a mechanical nature, and for two reasons. In the first place, it is not usual that among those who are content to devote themselves to routine factory work the specific aptitudes which they may possess will be overlaid and partially concealed by any considerable intelligence; while, in the second place, their cultivated interests may be so few and feeble as to leave these specific aptitudes as the sole peaks of eminence in the monotony of their comparatively stunted mental life. More than anything else, the presence of marked intelligence and strong interests complicates the exact estimation of the simpler mechanical aptitudes; moreover, the existence of a strong fund of intellectual energy is the most serious disqualification one could be burdened with in the attempt to achieve a lasting adaptation to much of our present-day factory life. The machine feeder, for example, must not be too intelligent, or he will quickly exhaust the interest of his work, become restless, dissatisfied, and a nuisance to his supervisors, or begin to look out for new employment. Ideally, then, the factory operative, according to the view of the complete Taylorist, will be just a semi-automatic attachment to the machine which he or she feeds, a 'hand,' to use a much-discredited term, rather than a 'brain' or a 'will.'

In support of the view that little intelligence is required for a considerable amount of present-day work the following paragraphs from a dialogue to be found in Link's *Employment Psychology*[2] may be quoted:

> *Mr W.* What became of the morons[3] you found?
>
> *Miss N.* We assigned them to some simple machine-work and they turned out to be excellent operators. They were very quiet and steady. One of them by the way we discovered by her attempt to do the *hard-directions* test. She stumbled along till she got to the direction "Tell where the sun rises, in the east or in the west?" She pondered over this for a while and then remarked, "That's a hard one." "Why, don't you know where the sun rises?" asked the examiner. "No, I used to know but I forgot," was the answer. Yet we found that this girl had the ability to do something and do it well.

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[1] Taylor, *Principles of Scientific Management*.

[2] Pp. 136–137.

[3] *Moron*: a person of defective intelligence.

> *Mr W.* But how about the higher grades of intelligence?
>
> *Miss N.* There again we find that no matter how high a person's general intelligence may be, it does not follow that he will be especially intelligent in a special kind of work. For instance, another girl who took the test I mentioned came to the same question and put down as her answer "The sun does not rise, but the earth revolving on its axis makes it seem as if it came up in the east." This girl was a high-school graduate and above the average in intelligence, but she did not make good at the clerical work for which she was hired.

It may be added that Link's work admittedly became increasingly difficult as he moved away from the level of the routine worker, and attempted to deal with those of a more intelligent type.

While, then, we may say that considerable success has marked the efforts of Taylor and his successors to make workers of a low-grade type fit into positions in the industrial machine suitable for the functioning of their almost mechanical aptitudes, can we reasonably hope that through the application of psychological tests we shall be able eventually to fit the worker with higher qualities of mind into the sort of position where live interests will not interfere with his efficiency, and a high degree of intelligence will be a real asset?

II.

In investigating this possibility we shall find that what has been so far done in the selection of workers for tasks other than those of a routine nature is less unique than at first sight it seems to be. Thus, Mr Cyril Burt[1] has pointed out that Münsterberg's famous car-driver test did not so much discover the men, and those only, with the peculiar qualities needed for securing a rapid and accurate adjustment to the changing situations of streets, as those with just an undifferentiated general ability which might have been equally well applied elsewhere; moreover, the fact that the college students who also did the test came through, on Münsterberg's own admission, with better results than the most highly-trained drivers, would seem to support this view. If, then, this contention of Mr Burt, and similar contentions, are reasonable, what we have been doing in the selection of workers above the merely routine level, of salesmen and clerks, of car-drivers and telephonists, of army officers and others in executive positions, and so on, amounts to no more than the selection of those among our subjects who have possessed, not special aptitudes, but the greatest amount of general ability. In short, we have weeded out not the *misfits*, but the *unfits*. The American army psychological tests may be quoted as the supreme example of this method of choosing men for highly specialised work by virtue of their possession of an all-round general ability which, it is reasonable to suppose, could be utilised in a variety of circumstances with equal success.

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[1] See *Industrial Administration* (Pitman and Co.), pp. 90–91.

An indispensable and essential preliminary, therefore, to a correct psychography of the vocations—without which vocational selection can only take place in an extremely unscientific fashion—is a satisfactory determination of the relations which hold between general ability and the specific aptitudes called for in the performance of particular tasks. Till this is done, we shall be greatly handicapped. Thus it will be comparatively futile to conclude that a salesman needs, let us say, a good memory and then proceed to test his memory for nonsense syllables without knowing either whether we are testing exactly the same aptitude as functions in his work or whether it is independent of general ability or not.

Until the psychography of the various industrial occupations is successfully accomplished, however, there can be little doubt that it will be advisable to construct vocational tests in such a way as to reproduce as nearly as possible, while allowing for practice effects, the mental conditions of the work for which we may be attempting to diagnose fitness but without stumbling into the pit which Münsterberg apparently could not avoid.

The method most popular, I believe, in America at present differs from this; it is to try out a number of miscellaneous standardised tests, to reject those which show no correlations with known ability for any particular vocation and to retain those which grade actual workers in the order of their trade ranking. Thus, it has been found that the *Completion Test* produces a correlation of + ·63 with engineering ability, and the *Knox Cube Test* one of + ·73 with aptitude for label-pasting[1].

It is difficult, however, to feel quite satisfied with this method. One cannot escape the conviction that either general ability or purely accidental causes were responsible for such a high correlation between such dissimilar activities. If, for example, it should be found that in engineering a turner's ability corresponds closely with his accuracy in checking numbers but that another test which reproduces with greater faithfulness the essential mental conditions inseparable from a turner's work is equally significant, then we must inevitably have greater confidence in the latter than in the former. In the process of turning, the accuracy of judgment demanded is displayed not continuously as in number-checking, but at the end of long periods of concentrated attention, so that a test which calls for the accurate mechanical adjustment of a pointer to the surface of a rotating drum at the moment when a slowly approaching mark comes directly under it will be much more promising than a test involving an acquaintance with words, numbers, and abstract ideas. We may still be testing general ability but there is at least a chance that we shall be testing specific aptitude in addition. (But even in applying such a test as this we should discover that another important factor, *pride in a finished job*, is not being sufficiently evoked. The chief satisfaction a good turner derives from his work is in seeing the spindle, let us say, which, like a sculptor, he has brought out of the rough, lying shining and neat before him.) The point to remember is that the farther we get away from the activities actually employed in the work the more likely it will be that we shall miss whatever specific aptitude may be demanded in the actual task.

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[1] See Hollingworth and Poffenberger, *Applied Psychology*.

This method of constructing 'imitative' tests is advocated because it obviates for the present the necessity for awaiting a clear understanding of the relationship between general ability and the specific intellectual aptitudes, while unconsciously through its use the skilful investigator for whom psychology is an art as well as a science will be able to bring into operation mental factors definitely essential but not yet revealed by conscious scientific analysis. Accordingly, Link's practice of working himself at the tasks for which he was devising tests is the model procedure strongly to be recommended.

One cannot help feeling that the method of Seashore in investigating separately—if this, indeed, is possible—all the qualities necessary for success as a singer, or of Münsterberg in testing telephonists in a similar manner is open to objection and has only succeeded in their hands because its defects were unconsciously made good by the presence of unusual insight and skill.

Suppose, for example, we test two telephonists for sight, hearing, accuracy of co-ordinated movements, immediate memory, and general ability. To proceed, as Münsterberg did, merely to add up the scores in each test as they stand is to assume that all the qualities are equally important. How they function in proportion to one another in the actual work may be quite another matter. If we assume, however, what is not unreasonable, that poor sight may be remedied by glasses, and that intelligence will often fill in auditory blanks, and if we accordingly *weight* the scores for immediate memory and general ability in a suitable manner, we may actually produce in certain cases a *reversal* of the order of ability which the former method, as Münsterberg employed it, gives. Experiment, however, is necessary before this matter can be cleared up satisfactorily.

III.

The vocational psychologist has so far, I believe, been tempted to proceed with his work on the assumption that intellectual abilities are all-important as qualifications for vocational fitness. I believe that we shall only be able to get a deeper insight into our problems through the systematic employment of some such analysis of our human abilities as takes into account not only these but other psychical factors as well. Thus to attempt such an analysis:

Man may be regarded in two ways; as a *creature* capable of adapting himself to the environment in which he finds himself placed, or as a *creator* capable of moulding his environment to suit his will. Under the aspect of *creature* he may be looked upon, in the manner almost traditional now, (1) as a mechanism to be studied through psychophysics, (2) as an organism with his perceptions and impulses coloured by his instinctive preferences and antipathies, or (3) as a rational being capable of deliberation and foresight; while under the aspect of *creator* he will appear as the master of these means to his self expression, capable as a *super*-mechanism of creative artistic work (but, when little more than a *super*-mechanism, of excessive preoccupation with delight in the mastery of his materials); as *super*-organism of ideal devotion and purpose, or as a *super*-rational being of intuitive insight and understanding[1]. In this connexion the conceptions, with which we have recently become familiarised, of a g*, of a w†, and of a c‡ as factors combining to make up total personality, may be utilised as aids to the explanation of such a scheme. We may thus set out the above ideas (see diagram on p. 201).

In the light of this diagram it will be clear that the perfect intelligence test will call into play the power of mastering new problems as well as of dealing effectively with old ones, of exhibiting interests and representing instinctive motivation as well as mechanical efficiency. Thus we may define intelligence as the general capacity of an individual to adjust his thinking both to familiar and to unfamiliar requirements of his environment whether it is that to which he is accustomed or unaccustomed, and to mould it when necessary to suit his needs.

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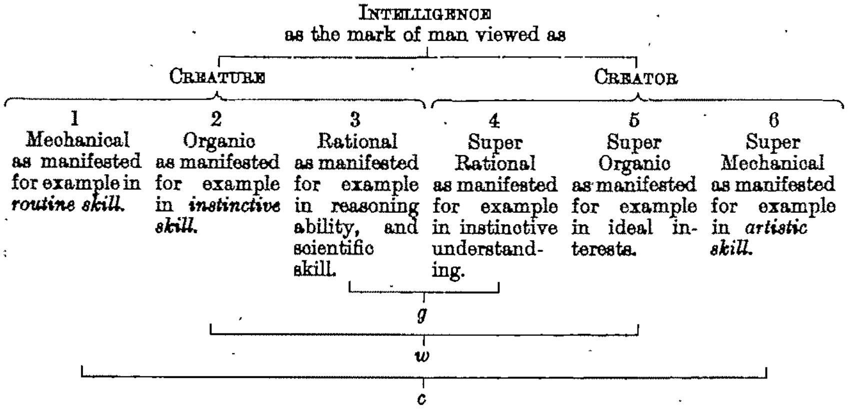
[1] This analysis (slightly modified here) is to be found in Mr Benchara Branford's *New Chapter in the Science of Government*, § 19.

* The 'general factor' of Prof. Spearman. *Br. Journ. Psych.* v, No. 1.

† The 'persistence' factor of Dr Webb. *Br. Journ. Psych. Monog.* No. 3.

‡ The 'cleverness' factor of Mr J. C. M. Garnett. *This Journal*, IX, Nos. 3, 4.


INTELLIGENCE
as the mark of man viewed as
CREATURE
CREATOR
1
Mechanical
as manifested for example in *routine skill.*
2
Organic
as manifested for example in *instinctive skill.*
3
Rational
as manifested for example in reasoning ability, and scientific skill.
4
Super Rational
as manifested for example in instinctive understanding.
5
Super Organic
as manifested for example in ideal interests.
6
Super Mechanical
as manifested for example in *artistic skill.*
g
w
c

From this point of view the American army tests will be seen clearly to be not completely satisfactory for the diagnosis of the higher forms of intelligence; it is the correct but stereotyped rather than the original and creative response which they call out. Moreover, through the time-limit imposed for their performance, they over-emphasize the factor of speedy mental *tempo* in pencil and paper work. Again, viewed in the perspective of our diagram they will be seen to lack any appeal to the *w* qualities, while it will be generally admitted that they place workers who are not engaged in clerical occupations at too great a disadvantage. They are excellent, no doubt, for the discovery of those persons possessing the ability to fill minor executive positions in which accurate comprehension of instructions, and promptness and faithfulness of despatch are demanded, but they would only reveal by accident, if at all, a Newton, or an Edison, of comparatively slower reaction but enormously greater intelligence than the first-class clerk possesses.

From this criticism it follows that from performances in a test involving correct reactions of a predetermined kind we can get no more than shadow pictures of *creative* intelligence, and after all the best intelligence test will be the one which allows for the display of the greatest possible degree of intelligence and not less. Cancellation, number-checking, and association tests fail to come up to this requirement, and consequently we cannot expect that they will avail to sort out for us our Darwins from our well-educated dunces. Speaking generally we may say that the disciples of Taylor have constructed tests for the intelligence shown in column 1 and the American psychologists tests for that shown in column 3.

In vocational work, then, we shall need to make a wider and deeper appeal in our tests to make them successful with workers above the routine level. To quote an example. We may give a simple *form-board* test to fitters and find perhaps that the ability to recognise and place familiar geometrical shapes into recesses shaped to receive them is a good indication of routine fitting ability, but since it does not appeal very strongly to the *interest in making things work,* which after all is the motive force driving the better boys into fitting as an occupation, it cannot be the test most serviceable in the long run. A boy's ability, however, in constructing, so that it will work, a simple arrangement in which certain mechanical principles are utilised, as for example in a bell of novel variety, is a completer indication of fitting ability, since constructive ability which has an instinctive basis and involves more than the *g* activities is here called for. I have found that such a test as this correlates with the trade ability of fitters as closely as + 0·6 while the *form-board* test produces a much lower and usually insignificant correlation. The putting of things together with constructive skill measures, that is to say, a greater common factor of the fitter's intelligence. Link has found, it may be noted, that a mechanical puzzle test such as I have described is usually *too* difficult for those engaged in merely *routine* fitting.

No real progress will be made, I believe, with the application of psychological tests to workers on non-routine tasks involving specific aptitudes and interests unless, then, we learn to appeal not only to the intellectual qualities but also the *c* and *w* qualities. We may discover by intellectual tests good fitters who fail subsequently because of temperamental defects. It would be fatal to predict for one who scored 80 per cent. in tests like the American army tests a greater success in a sphere where initiative and patience are required than another who scored 75 per cent., if the latter should happen to possess a big fund of curiosity and constructive interest.

It must certainly seem that the vocational psychologist will need to take into consideration what has so far been overlooked—the instinctive make-up of the subjects of his experiments, and that in choosing workers for particular tasks he must make sure that their instinctive type is the right one. It would be folly to place the man with strong gregarious tendencies on lighthouse or wireless work, or advise him to go farming in a new colony, just as it would be unwise to encourage a boy devoid of the acquisitive instinct to set up in business. And it will be agreed that a soldier or a reformer without a considerable spice of pugnacity would probably prove a failure, while it is equally beyond dispute that the statesman and the priest, the doctor and the nurse, the teacher and the shepherd, would soon tire of their work if they were not endowed strongly with the tender impulse. The next few years will show whether we shall be able to devise successful tests for the diagnosis of the relative strengths of man's various instinctive qualities.

Meanwhile something may be done by the utilisation of tests already in existence which have not been so far employed to any extent in the investigation of certain instinctive character qualities. For example there is a test in Whipple's *Manual*[1] which has not been exploited to anything like its full possibilities if at all, for vocational purposes. This is the well-known *Testimony Test.* The method of its application is probably familiar to everyone. A subject is shown an interesting picture which he studies carefully and in detail for two or three minutes, and afterwards answers from memory a series of questions concerning it. Many of the questions are deliberately framed to confuse and mislead him. During the cross-examination the subject may indicate the degree of his felt certainty in the reliability of his replies. He may use the numbers 1, 2, 3, and 4 for this purpose. Thus he marks his answers with a 1, when quite uncertain whether they are right or wrong; with a 2, when there is a hesitation to commit himself too far; with a 3, when he is positive that he is right; and with a 4 when he feels he could safely take an oath that they are correct.

The value of the test for the determination of character qualities depends upon the following theoretical presuppositions which experiment largely justifies:

(*a*) The degree of readiness of a subject to commit himself is a rough measure of his general decisiveness in situations where there is a common tendency to hesitate. When justified by results this is an invaluable quality for executive work.

(*b*) A person of little reliability commits himself too freely, *i.e.* in the absence of valid reasons. Such a person is obviously unsuited for a position of responsibility.

(*c*) Equally unfitted, also, for responsible work is the person who is too cautious or timid, who has good grounds for holding decided opinions but rarely or never commits himself to the full, so that the figure 4 is practically absent from his answers list.

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[1] *Manual of Physical and Mental Tests,* II.

In applying this test (in conjunction with a colleague[1] who suggested its use) to the head of a department in a big store we found that whereas she made an excellent score in 'following instructions' she was unwilling to commit herself to any marked extent in the testimony test. This avoidance of decisiveness was also supplemented by a disinclination to tackle questions of a problematical nature when others of a routine nature could be answered instead. We, therefore, formed the opinion that while she might make a successful head of a department in a business where each department depended for its prosperity upon the wider efforts of the management, so that she would be floated along, let us say, on the wave of the firm's general efficiency, she would be unsuited for *pioneering* a new venture outside. No such judgment could be made on the basis of the results of tests involving nothing but intellectual qualities. It was interesting to find that the managing director was willing to accept our estimate as probably correct though he had himself formed quite a different judgment of this lady's capabilities. But he recalled the details of several cases where men had failed in outside independent ventures, and failed more than once after a long record of successful management within and he was willing to believe that we were probably right. The testimony experiment may, therefore, prove invaluable to the vocational psychologist.

In conclusion I may be permitted to mention and describe a test of my own which has proved equally serviceable. It will be interesting and this must be my justification for mentioning it, because it introduces, I believe, a principle into vocational psychology, which has not been consciously employed hitherto, the principle of the *optional question*. I have found that by the employment of the choice principle a test can be constructed which looks like an intelligence test and so takes the 'censor,' to use the Freudian metaphor, off its guard, but which is in reality quite as much a revelation of the character as of the intellectual qualities. The test takes at least half-an-hour to complete and there is no time limit imposed. Ten questions have to be answered, one on each of ten pages. On each page three questions are set out involving for their reply (i) a routine operation such as counting figures, or making a list of common articles, (ii) the accurate comprehension of instructions with the ability to follow them, and (iii) the tackling of a problem of a more difficult kind. It has been found in a large number of cases that in the absence of a time limit, subjects do not, as the non-psychological student might conclude, choose what may be regarded as the simplest questions. Subjects have almost invariably been found to choose the *questions which interest them most.* If the subject is instructed to mark with a cross the question which he would have answered had he not chosen as he did, we can get a shrewd idea, through the third and neglected questions, of the kind of thing he systematically avoids. I have managed to arrange, in addition to the choices mentioned above, choices between questions with an abstract and a concrete reference, between those involving an interest in the ego and in general ethical principles and between those involving an appeal to various specific aptitudes. It is a test which may, therefore, be constructed so as to give us a good general picture of a subject's mind in its broader outlines. The psychology of the optional question[1] has not been seriously studied. So that even if a more extended use of my particular type of test should prove unfruitful, the conscious and systematic application of the optional question principle should open up a way into the province which we are all eager to enter, where the secrets of personality lie concealed.

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[1] Mr E. R. Mason-Thompson of the Pelman Psychological Laboratory.

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[1] I am indebted to Mr Cyril Burt for this phrase used in connexion with the test first described.

PSYCHICAL SELECTION: EXPRESSION AND IMPRESSION

BY C. LLOYD MORGAN.

1. *Introductory* (pp. 206, 207).
2. *Relation of Expression to Impression* (pp. 207–209).
3. *Imitation. Behaviour with and without Intention* (pp. 209, 210).
4. *Similar Expression with different Impression* (pp. 210, 211).
5. *Similar Expression in different situations* (pp. 211, 212).
6. *Behaviour in 'Courtship.' Is it primarily intentional?* (p. 213).
7. *Two sub-classes of secondary sexual Characters. An example from Insect life* (pp. 213–215).
8. *A 'taste for the beautiful'* (pp. 215, 216).
9. *The stress on 'recognition'* (pp. 216–218).
10. *The Expressive Behaviour of the Male* (pp. 218–221).
11. *The coyness of the Female* (pp. 221, 222).
12. *Conclusion* (pp. 222–224).

1.

WHATEVER may be said for and against the hypothesis of natural selection, this is not the place to defend it. Properly safeguarded I for one accept it whole-heartedly. Here I am concerned with psychical selection. In some sense of the phrase it has unquestionably been a factor in evolutionary progress. I propose then to discuss what seems to be its chief relational feature. This will involve a reconsideration of some of the data afforded by sexual selection. But for our present purpose, and in this *Journal*, the emphasis falls on psychological factors. And since psychology does not deal only with mental processes in man, since, too, salient examples of that which is to be considered are afforded in the life of birds, most of the illustrations will be taken from their behaviour and what seems to be implied thereby.

Some reference will be made to the emotional qualification of consciousness or, as I prefer to say, of enjoyment. Fully realising the great value and importance of Mr Shand's treatment of the emotions as conational complexes definable by their ends, I here regard them as affects, describable only as what they feel like, though the concrete instances of mental process in which their presence may be detected are no doubt cognitive and conative as well as affective. I am not here concerned, however, to distinguish their several qualities, save incidentally in a quite perfunctory way. This does not fall within my picture. But a more general question is pertinent. What is the good of emotion—not of this or that quality of affect—but just of emotion as such? What utility can be assigned to it? For our present purpose, without staying to defend the position, I define emotion as super-normal enjoyment, positive or negative. And the good of it—its utility from the evolutionary point of view—is to modify the behaviour of the organism with whose functional activities such super-normal enjoyment is correlated. Behaviour is generally enhanced and rendered more strenuous; sometimes its level of vigour is depressed; and occasionally, in abnormal cases, its effective integration is interfered with; but in these last cases its biological end is defeated. It need hardly be said that there is in this broad view nothing to preclude differentiation of behaviour in accordance with the specific quality of the emotional affect in correlation, let us say, with the 'hormonic' balance in the organism.

A further introductory sentence must be added. I use the word 'instinctive' as adjectival to behaviour (or to a disposition to behave) in so far as such behaviour has not been learnt in the course of individual life. It is, however, in this usage, very often only an instinctive *factor*, which, though inseparable, is distinguishable, under analysis, from the acquired factor due to individual learning and profiting by experience. I think that in reference to behaviour, and its correlated enjoyment, this usage may be retained without prejudice to the different usages of Prof. McDougall, of Dr Drever, and of others whose universe of discourse is in one sense narrower, and in another sense wider, than that of the following discussion; narrower in so far as it is mainly concerned with instinct in man; wider in so far as it is applicable to more than behaviour with its specifically correlated enjoyment.

2.

In the life of birds the behaviour is apparently similar under different circumstances—for example when intruders of the same or of allied species enter the breeding territory, when so-called courtship is in progress, and when the young are disturbed. In each situation the bird appears to be in a state of excitement which we interpret as emotional; but the emotional qualification of enjoyment seems to us to be anger in the first case; to be that which accompanies strongly felt sex-hunger in the second case; and to be something of the nature of distress in the third case. What we have to lay stress upon is the similarity in the type and intensity of the response at different emotional periods. Here the question of biological value naturally arises. Is the similar behaviour contributory either to race-maintenance, or to self-preservation, or to both, under all the different circumstances in which it is observed? Those who assert that only that structure or that behaviour which has utility can escape elimination in the struggle for existence, seem bound to contend that the behaviour must be useful at some period of life; but there seems no reason why we should reject the hypothesis that the same type of behaviour may be useful in different ways in different situations. As Mr Eliot Howard says in his *British Warblers*: "If it can be shown that there is a strong probability of some specific type of behaviour serving a double or a treble purpose, so much the more reason is there for its survival. The reactions that we meet with in bird life may thus be said to have developed firstly as a means for arousing the requisite amount of pairing hunger in the female, secondly as a warning to intruders; and thirdly as a protection for the helpless offspring. Clearly everything centres round the strength of the probability in each case[1]."

In the third case to which Mr Howard here refers, the protection of the helpless offspring is afforded indirectly. The behaviour of the parents is such as to draw to themselves the attention of intruders, and thus to divert attention from the young, which are thus preserved from injury. Now we may speak of the behaviour in all three situations as an *expression* of a disposition which is emotionally toned. And in all three situations the suggested utility of the expression—that utility of which we seek evidence—arises from the *impression* which is produced thereby—in the first by arousing the requisite pairing hunger; in the second as a warning to intruders; and in the third by diverting attention from the helpless young. I propose, therefore, to use the word 'expression' for that which is correlative to 'impression,' on the understanding that such impression is normally provocative of behaviour in the organism which receives it. On these terms expression is a sub-species of behaviour differentiated from other modes of behaviour in that its utility lies in the impression it produces on some other organism; while the impression is a sub-species of presentation differing from other presentations in that what is presented is expressive behaviour on the part of some other organism. If this usage of the words be accepted, the points for emphasis are (1) that what is apparently the same expression is expressive of different emotional states, and, further, (2) that the impression produced is different in different situations. On the thorough-going doctrine of utility there must be some biological value in each situation. But not all behaviour is expressive in the sense suggested. It seems that, in bird life, there are many activities which are not expressive on these terms. Birds are very active in the early hours of the day, especially during the breeding season. But how much of their procedure is expressive, in the sense that the utility, or even part of the utility, of what they do lies in the impression it produces on another bird, or animal, and, through that impression, influences *its* behaviour? Not all certainly; still, some of it is thus expressive and does produce an impression; and that is what we are here concerned with.

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[1] Part IX, 8, 9.

3.

Passing reference to imitation is here in place. Setting on one side what Prof. Mark Baldwin speaks of as 'self-imitation,' it takes at least two to constitute an imitative situation. In such a situation there is behaviour on the part of *A*; and this behaviour is expressive in our sense in so far as there is an answering impression received by *B*; and the behaviour of *B* is imitative in so far as it resembles the expressive behaviour of *A*. Imitation is therefore a specialised case of the relation of expression to impression. Since, however, there are those who contend that imitation, *sensu stricto*, implies intention to imitate, it should be understood that I here use the word as inclusive even of what the biologist may term 'mimetic' behaviour where intention is absent. Are there then cases of imitation in a broad sense which are to be regarded as instinctive? From my point of view there can be no doubt as to the answer. There are forms of imitation in this broad sense which have not to be learnt but just come by nature.

Now perhaps it will be said that if we rule out intention we thereby place such so-called imitation altogether outside the universe of psychical regard. But is this so if the impression is itself psychical in its nature? I assume that as an enjoyed presentation it is so. And I urge that, if so, the expression which gets its value as such in relation to the psychical impression is thereby drawn within the field of psychological regard. That is why I lay stress on expression as, for our purpose, to be taken in its relation to impression. They are correlative terms in one field of relatedness. None the less the distinction between behaving with intention and behaving without intention is important. It must be steadily borne in mind. And this is the more necessary because so much of our own behaviour is in relation to foreseen ends, and because the consequences of our own acts are so often quite definitely and deliberately intended. But even with us surely not always. And in the case of animals purely instinctive acts (if such there be) and much of their behaviour, in so far as instinctive and unlearnt by the individual performer, afford, as I think, no evidence of an end in view such as intention involves. Professor Stout would say that a very vague and indefinite end in view is always present. Let it go at that. Practically (I do not say theoretically) there is little difference between what I mean by absence of intention and what he claims as present in so nebulous a form.

We must realise then that, in some measure in human life, and probably in far larger measure in animal life there may be expression which produces impression in the complete absence (for purposes of practical interpretation) of any intention to produce that impression; and that this is especially the case in so far as instinctive factors of behaviour are concerned. If it be granted therefore that apparently similar behaviour at one time warns off an intruder into the territory, at another time stimulates the pairing hunger of the hen bird, and at yet another time attracts the attention of an animal that too closely approaches the young, there need be no intention to warn, or to win the mate, or to draw off from the offspring, still less, in the last case, to delude by simulating helplessness. On my view there is no such intention so far as the behaviour is instinctive, though it may be present so far as acquired experience is co-operant. So too the impression received need be no more than provocative of responsive behaviour which the recipient feels like carrying out.

4.

Let us, then, take expression and impression as correlative; and let us assume that for behaviour to be expressive there need be no intention to make an impression on, say, some other bird, and that behaviour may instinctively be the outcome of impression without any intention so to behave. We may now ask how we can account for the fact that, so far as observation goes, the same or very similar expression in behaviour seems on different occasions to produce a different impression, or at any rate one which leads to very different behaviour on the part of the recipient. I do not think this presents much difficulty. In the first place the observations which Mr Howard describes are made on adult birds, and, in them, what we speak of as the impression carries meaning which has been acquired in the course of previous experience. Indeed it is only with some difficulty that we can realise that there may be impressions, and other presentations in naïve awareness, which have not put on the vesture of acquired meaning. And so far as such meaning is present, there is no difficulty whatever in understanding how closely similar impressions may be differentiated on different occasions. Let us however consider what is actually presented, say to vision, on the several occasions. Is it so similar as at first sight it may seem? By fixing our attention on the expressive attitudes and movements *we regard them in isolation from their context*. But as they are presented to the recipient of the impression they are nowise thus isolated. They are set in the context of the situation as a whole. More than we readily realise the animal, like the little child, is responsive rather to the total presented situation than to the details—even the salient details—which we, for the purposes of our thought, distinguish and isolate. We concentrate attention on the similarity of behaviour because that is what we seek to interpret; and yet we see clearly enough that in each of the three cases to which Mr Howard refers, the circumstances differ notwithstanding the similarity of expressive behaviour in their midst. The situations are clearly differentiated for *our* observation. Why should not the bird, after his naïver fashion, feel this difference and act accordingly? Why should not the context play its part in determining *his* behaviour? We must remember, too, that living through, and behaving in the midst of, a given situation begets an attitude of mind, a progressive differentiation of enjoyment, in relation to just that situation. The outlook of the bird is brought into harmony with that upon which he looks out. Not only therefore is there difference in the world context in which the expression finds its place, there is difference also in the mind context in which the impression finds its place. During the period of so-called courtship the male bird, and probably the female too, are, very markedly, as it seems, under the influence of sex-hunger, and we may well suppose that specialised interest colours the whole situation and gives at that time special meaning to expressive behaviour, which, seen at other times, would carry a different meaning.

5.

But how are we to account for the similarity of expression in different situations? Why should there be much the same behaviour when another male enters the breeding territory, when mating is in progress, and when the young are disturbed? In the first place it should be remembered that the modes of behaviour now in question are chiefly in evidence during the period when dispositions subsidiary to race-maintenance find expression in modes of action common to this period under the physiological influence of the internal secretions of the group of related glands in the organic sex-complex. If this common expression have diverse utility in different situations, like factors therein would be emphasized. The whole period is one of unusual excitement—*i.e.* emotion as super-normal—and it may be that the readiest outlet of the motor effects of this excitement is along certain predetermined channels. One is here reminded of Herbert Spencer's doctrine that an overflow of 'nervous force' takes first the most 'habitual' routes. And one knows that in the case he discussed in advocating this view[1] laughter may be the outcome of very different kinds of emotional excitement, often, as we commonly say, of mere high-strung nervous tension. It may be, therefore, that the similar behaviour in different situations is an index of a general type of excitement under a ruling disposition, and that we must, and practically do, infer the specific quality of the emotional affect rather from the context of the behaviour than from the behaviour itself. There may, too, be subtle differences which our powers of observation are not sufficiently delicate to enable us readily to distinguish—differences analogous to those between the genial laugh of good fellowship, the laugh of exultation over a fallen enemy, and the laugh of irritation when, say, a golfing opponent has his usual persistent and undeserved good luck. These differences we learn to appreciate because we too are human folk and grasp the nuances of the situation. May there not be differences which birds sense better than we do, just because they *are* birds? Such modes of expression as we are considering, in their relation to impressions received, whether they are really alike on different occasions, or really slightly diverse and only apparently the same, betoken, as has been said, periods of excitement. That excitement should find an outlet in vigorous behaviour has biological value and hence utility in a broad sense. It is a relief from tension and strain which might otherwise be harmful to the organism. If there be also specialised utility in affording an impression which stimulates another bird to behaviour appropriate to the situation, there do not seem to be insuperable difficulties which preclude an adequate interpretation of the facts.

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[1] *Essays*, I, 194.

6.

Let us now consider in further detail the bird's behaviour during the period of so-called courtship. It may be well first to say why I speak of *so-called* courtship. I take it that the word 'courtship' may naturally be held to imply intention on the part of the male so to act—so to sing, for example, or so to display his plumage—as to make himself attractive to the hen-bird he 'seeks to win' as his mate. Now of course such intention may be present. There may be in the mind of the male something of this sort: "By such and such expression an impression will be produced; and the production of this impression will contribute to the end I have in view." It may be vaguer than this—so vague that one can hardly put it in words; he may just realise in some dim but sufficient way that what he does has its effect and intends to do it for the sake of this effect. On the other hand it may be urged that what is *necessary* for an interpretation of the observed facts is less even than this. All that is required, on this view, is that, in accordance with his disposition at the time, the male feels like behaving in certain ways; that this behaviour does produce a stimulating impression; and that, on the receipt of this impression, the hen-bird, in accordance with her disposition at the time, feels like mating with him. We will provisionally assume that only this minimum is present so far as the instinctive factors in behaviour are concerned.

It may be said that no act in adult life *is* purely instinctive. When the bird has reached the age of 'courtship' there is always in addition to, and intimately combined with, the instinctive and unlearnt factor in behaviour, some legacy from the lessons which have already been learnt. That is so; and it is just this which renders interpretation peculiarly difficult. My point is that, so far as we can get at the facts by observation, there is in the male bird's procedure much that is seemingly unlearnt—much that seems just to come in presence of the 'courtship' situation when first it is presented. And I urge that, *so far as this unlearnt part is concerned*, intention so to act as to attain results of which there has been no prior experience, is presumably absent.

7.

We must now come to still closer quarters with some of the phenomena connected with the life of birds in the period of 'courtship.' In the Second Part of his great work on *The Descent of Man*, Darwin very fully discusses the evidence for his hypothesis of sexual selection as a factor in evolution. How does he differentiate it from natural selection or, as he sometimes puts it, from other forms of natural selection? It depends, he says, "on the advantage which certain individuals have over others of the same sex and species solely in respect of reproduction[1]." This advantage has specialised biological value to the end of mating, and hence there have been developed secondary sexual characters which in their present form are in part due to this selection.

Now it is clear that the secondary characters developed, as Darwin claims, through sexual selection, fall within two different sub-classes; (1) those which are not primarily dependent on the impression they make on the mate; (2) those which *are* thus dependent since they allure or excite an individual of the other sex. Those in the first category are of advantage in the struggle to secure a mate; but they are not necessarily of advantage through any appeal they make to that mate. It is not through her preference under psychical selection, that the best fighter wins. She may *also* prefer the winner; but she must often yield to the victor whether she prefers him to others or not. She must often yield to him who can capture and hold her. She is just a prize to the strong, and has, so to speak, no voice in the matter. But under the second category the advantage lies with the possessor of those organs and structures, used to the best effect, which so allure or excite the female as not merely to force but to induce her to mate.

Since from the point of view here taken the whole matter turns on the relation of expression to impression it may be well to consider briefly a simple but in some respects typical example in insect life. "The female of the Emperor moth (*Saturnia carpini*) is so eagerly sought by the males that when a virgin female is taken into a favourable locality, the collector is soon surrounded by troops of males which have been guided by a marvellously delicate sense of smell residing in their branching antennae. So delicate is the sense that the female is recognized perhaps miles away, and recognized as a virgin[2]." Now here the scent emitted by the female is a form of expression which produces a presentative impression on the males through receptors in the specially developed antennae. And there seems little doubt that the biological value of expression and impression here centres in, if it be not exclusively related to, reproduction. If this be so, the possession of specialised antennae may fairly be regarded as a secondary sexual character in the male. It will, no doubt, be said that there is here no evidence of preference on the part of the female; she does not select the male with the most branching antennae. Still the case does fall under Darwin's principle as stated by him; and it does fall under the second category, for it involves alluring and exciting an individual of the other sex. Now let us suppose that, of two females, one produced the expressive scent in greater abundance than the other. That one would have a greater and more far-reaching effect. Some distant male, perhaps with better developed antennae than others, would receive an impression from the one female and not from the other, or perhaps a stronger impression from the one than from the other. May we say that if he flies to the one and not the other he chooses her? And can we say that in any valid sense she chooses him? Some will say: most certainly not. Choice is choice; and this is not choice. It is simply behaving under the stimulating influence of what is assumed to be the stronger impression. Yes! And that, I venture to suggest, is just what Darwin meant (even in the much more complex cases of bird-life) by speaking of the females as 'unconsciously preferring' certain males which most strongly excite them. "It is not probable," he says, "that she consciously deliberates; but she is most excited or attracted by the most beautiful, or melodious or gallant males[1]." One must remember that at our human level of mental development, choice is for the most part purposeful. As we commonly use the word there is nearly always implied a foreseen end for the sake of which the choice is made. At the much lower stage of mental development we are trying to read ourselves into—with difficulty since we must leave so much of *ourselves* out of account—at this stage what we are forced by the exigencies of language, fashioned to deal with human affairs, to speak of as choice or preference, may be much more naïve—so naïve indeed as to be reduced to the level of the relation between adequate stimulation and adaptive response, if, but only if, we deliberately turn our gaze away from the correlated psychical aspect.

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[1] p. 322.

[2] Poulton, *Colours of Animals*, p. 288.

8.

Another verbal question—which like the others is really more than merely verbal—calls for passing comment; and then we can proceed on our way. In many passages Darwin speaks of a 'taste for the beautiful,' and uses the word 'aesthetic.' Thus he says "On the whole birds appear to be the most aesthetic of all animals, excepting of course man, and they have nearly the same taste for the beautiful as we have[2]." In what sense are we to understand 'taste'? The pilfering blackbird seems to have nearly the same taste for ripe fruit that we have. Is one misunderstanding Darwin if one supposes that he used the phrase 'taste for the beautiful' in an analogous sense? I take it that Darwin did not wish to raise subtle questions, but to indicate fairly obvious inferences from what may be observed as interpreted in the light of his hypothesis. Look up the word 'aesthetic' in some current dictionary. Chamber's *Twentieth Century Dictionary* lies at hand. There I find, *sub. verb.* "the feeling of beauty in objects; the principles of taste and of art; the philosophy of the fine arts." Which of these, thus or otherwise formulated, did Darwin mean? Surely, I venture to suggest, the first. What does the bird know of the principles of taste and of art? What of the philosophy of the fine arts? But I see no reason why we should not credit the Peahen, when the Peacock erects and spreads his splendid tail-coverts, with a qualification of enjoyment analogous to that which we too experience on sight of him, *so long as we leave it at that* and do not begin to discourse on aesthetic values, beauty for beauty's sake, and the rest. I may no doubt be reading out of Darwin part of what he really meant. Or it may be said that I am reading into him my own views. But he does seem to safeguard his position in many passages in just the way I venture to suggest. Take for example this one. "I refer only," he says, "to the pleasure given by certain colours, forms and sounds, which may fairly be called a sense of the beautiful. With cultivated men such sensations are, however, intimately associated with complex ideas and trains of thought. When we behold a male bird elaborately displaying his graceful plumes, or splendid colours before the female, whilst other birds, not thus decorated, make no such display, it is impossible to doubt that she admires the beauty of her male partner[1]." For the present it is enough to lay stress on some differentiating character of the presentation which leads to response. It is this which seems to be really essential. There is differentiation in expression; this produces differentiated impression; and this again leads to a responsive act. When this expression evokes response, while *that* does not, we infer from the response that the female 'chooses' *this* male in preference to *that* one. But the observed facts may be interpreted on the supposition that *this* as expression affords an adequate impression while *that* does not; and that true choice with an end in view is absent.

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[1] p. 643.

[2] p. 550.

9.

Let us revert for a moment to the Emperor moth. Prof. Poulton says, in the passage I quoted above, that so delicate is the sense of smell in the male that the female is recognized perhaps miles away and recognized as a virgin. This use of the word 'recognized' is quite common among naturalists. But this recognition is not that the nature of which is discussed by psychologists. The latter is generally held to imply prior cognition in the course of individual life. All that is implied in what is sometimes called instinctive recognition is such impression as shall lead to responsive behaviour.

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[1] p. 140.

It is well known that Wallace was unable to accept sexual selection. And it is well known how much stress he laid on 'recognition marks.' "Among birds," he says, "these recognition marks are especially numerous and suggestive. Species which inhabit open districts are usually protectively coloured; but they generally possess some distinctive markings for the purpose of being easily recognized by their kind, both when at rest and during flight[1]." "Among insects the principle of distinctive coloration for recognition has probably been at work in the production of the wonderful variety of colour and marking we find everywhere, more especially among the butterflies and moths; and here its chief function may have been to secure the pairing together of individuals of the same species[2]." He speaks of "the various sounds and odours which are peculiar to the male, and which serve as a call to the female or as an indication of his presence. These are evidently a valuable addition to the means of recognition of the two sexes;...and thus the clearness, loudness, and individuality of the song becomes a useful character, and therefore the subject of natural selection[3]." Now recognition marks, call notes, and song, are, in our terminology, modes of expression; and Wallace's contention turns upon the claim, not only that they give rise to impression, but that, under natural selection, their very existence as such depends upon this impression—otherwise how can they be regarded as recognition marks *evolved to this very end*? But they are useful to each species "in relation to the most important function of their lives[4]"—that of reproduction. And it is just in view of their relation to this function that Darwin places them in the category of sexual selection. It is in virtue of *this* that recognition marks to the end of pairing are differentiated from warning colouration to the end of survival—also the outcome of the relation between expression and impression. But Wallace went even further on his way with Darwin than I have yet indicated. "Anyone," he says, "who reads these most interesting chapters [of Darwin's] will admit that the fact of display is demonstrated; and it may also be admitted, as highly probable, that the female is pleased or excited by the display." That seems to give Darwin pretty nearly all that he claims.

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[1] *Darwinism*, p. 222. [2] p. 226. [3] pp. 283, 284. [4] p. 285.

I have sought to emphasize the measure of harmony that may be found in Darwin and Wallace in the light of the relation which obtains between expression and impression. The point of interest to the psychologist is that for both certain features of structure or behaviour appeal to the mind and that in the absence of this appeal these features would not be what they are and as they are. Their evolution depends on a psychical factor—on some impression which they produce. Regarding then expression as correlative to impression, it seems that both the biological and the psychological value of expression is dependent on the psychical factor I deal with under the name 'impression.'

The further biological problems do not here concern us. Wallace was not satisfied that differential appeal of expression through the impression it produces suffices to enable us adequately to interpret the origin of secondary sexual characters. That may well be so. But Darwin dealt rather with the selection than the origin of these characters in the sense in which Wallace discussed the matter. These problems are not in our present picture. This however may be added. Wallace was unable to believe "that all the females of a species, or a great majority of them, over a wide area of country, and for many successive generations, prefer exactly the same modification of the colour or ornament[1]." But he seems to have found no difficulty in believing that all the members of a species *recognize* the same modification of colour or of ornament. But recognition marks imply differential impression and differential response no less than secondary sexual characters.

10.

Now so far as the response is instinctive there is, on our interpretation, no intentional or deliberate choice on the part of the female. But what about the behaviour of the male? Few would be prepared to contend that beautiful plumage is an intentional expression on the part of its possessor. Indeed it may be said that only by an extended use of the word can it thus be spoken of as an expression. Still it must be remembered that the word is here used as correlative to an impression produced, and in this sense the extension of usage is justifiable. If once we disabuse our thought of the idea that expression must necessarily be intentional, there is no reason why the brilliant plumage of birds, the mimetic or warning colouration of insects, the nectar, scent, and conspicuous hues of flowers, should not be regarded as expressive, in so far as they lead to impressions received by sentient organisms, and in so far as the expression has in some manner been evolved in relation to the impression produced and the behaviour which results therefrom. What we may speak of as *structural expression*, then, is not commonly regarded as, in any degree, intended by its possessor. When, however, we pass to 'display' in behaviour the presence of intention is generally assumed. It is indeed this assumed presence of intention which has led to the popular emphasis on the *courtship* of animals.

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[1] p. 285.

One who differs from Darwin in his interpretation of animal life, does so at his peril. In this matter of display Darwin spoke with no uncertain note. "When we behold two males fighting for the possession of the female; or several male birds displaying their gorgeous plumage, and performing the strangest antics before an assembled body of females, we cannot doubt that, though led by instinct, they know what they are about, and consciously exert their mental and bodily powers[1]." The males "sedulously display their charms before the females[2]." They "endeavour to charm or excite their mates by love-notes, songs, and antics[3]." They "display their fine plumage and other ornaments with so much care before the females[4]." "Sufficient facts have now been given to show with what care male birds display their various charms, and this they do with the utmost skill. While preening their feathers they have frequent opportunities for admiring themselves, and of studying how best to exhibit their beauty. But as all the males of the same species display themselves in exactly the same manner, it appears that actions, at first perhaps intentional have become instinctive[5]." "Hence we may conclude that it is the object of the male to induce the female to pair with him, and for this purpose he tries to excite or charm her in various ways; and this is the opinion of all those who have carefully studied the habits of living birds[6]."

It is noteworthy that Darwin was generally careful to qualify his allusions to preference on the part of the females by using such a phrase as "prefer, or are unconsciously excited by the more beautiful males[7]." But with regard to the behaviour of the male he says roundly, on the same page, that "they display their colours with elaborate care and to the best effect." Still in one of the passages I have quoted he seems ready to admit that in so far as instinctive, the behaviour is no longer intentional. And we have on the other side the passage in which it is suggested that the male birds have opportunities, while preening their feathers, for admiring themselves, and for studying how best to exhibit their beauty. If this suggestion be accepted, the behaviour which is the outcome of this study is clearly so far learnt; data for the most effective manner of display are intelligently acquired. I cannot accept the suggestion even from Darwin. If we grant that the male engaged in the instinctive business of preening, is also admiring his fine feathers, what he sees in detail is very different from what the female will see when she views him as a whole from some distance. I regard it as extremely improbable that the male bird realises that what thus, *ex hypothesi*, pleases him in detail, will produce, under quite different circumstances, so favourable an impression on his desired mate as to induce her to accept him. I am unable to water down what is supposed to pass through his mind to much less, in essentials, than this; and this I think is quite beyond his powers of inference.

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[1] p. 325. [2] p. 341. [3] p. 561. [4] p. 629; cf. pp. 600, 672.
[5] p. 612. [6] p. 616. [7] p. 757.

I submit that the elaborate display, executed in substantially the same manner by all the males of the same species, may fairly be regarded, complex as it undoubtedly is, as quite unintentional. Behaviour is no doubt in delicate harmony with ornamentation. Darwin compares the plumage and the display of the Peacock and the Peacock Pheasant[1]. The arrangement of the ocelli is such that effective sensory presentation involves 'display,' in the one case when the male stands in front of the female, erects and expands the tail-coverts, and at the same time 'shows off' his rich blue throat and breast; and in the other case when he stands obliquely to the female lowering the expanded wing on the near side and raising that on the far side. In this attitude, the tail-feathers being also expanded obliquely, "the ocelli over the whole body are exposed before the admiring female in one grand bespangled expanse." By such expression is an adequate impression produced. If, with Wallace, we regard the origin and the special character of the ornamentation as independent of psychical selection, are we to regard also its specific manner of display as independent of such selection? If, as those who deny even 'unconscious preference' on the part of the female must urge, the so-called display produces no impression that leads to response, how are we to account for the differentiated manner of display in this species and in that? If on the other hand it be intelligently learnt in the course of individual life, to subserve the foreseen end, is there a satisfactory observational basis on which is founded the inference that such a process of learning does take place? To revert to Darwin's suggestion that male birds are observed to preen their feathers. Is it likely that the Peacock or the Peacock Pheasant, in preening his feathers and admiring the ocelli thereon, reaches the conclusion that, as seen from some distance by the female, *this* mode of behaviour will display most effectively the specific ornamentation in the one case, and *that* in the other? Moreover it does not seem that the males of different species *have to learn* how to display their plumage to best advantage; this appears to come by nature. In other words the form of the behaviour as a whole appears, on the evidence, to be instinctive.

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[1] p. 605.

11.

Suppose, then, we regard it as a tenable view that the unlearnt basis of display is primarily instinctive, or in modern phrase a function of the unconscious; and suppose we regard the expressive display as giving rise to impressions on the part of the female; what then? How does the business work? "The courtship of animals," said Darwin, "is by no means so simple and short an affair as might be thought[1]"; and again, "the courtship is, in many instances, a prolonged affair[2]." Furthermore "as the illustrious Hunter long ago observed," the female "generally requires to be courted; she is coy, and may often be seen endeavouring for a long time to escape from the male[3]." Here we have, in the germ, the interpretation so ably developed by Gross in his *Play of Animals*. We must, however, be content to deal, and that briefly, with the root idea, which is that the coldness or coyness of the female (which surely involves a psychical factor) is only overcome by the adequate effect of strong expression on the part of the male; each being reciprocally developed in relation to the other. This view now meets with wide acceptance. But we must bear in mind that, for quite a long time, the male is always ready and eager to pair. Such is his disposition while the hormones course in his blood. Not so the female; her readiness, perhaps her eagerness, to pair, comes in periodic gushes, and is possibly correlated with the liberation of internal secretions when the ova are dehisced from the ovary. If this be so—if sexual excitement in the female be periodic—this must be taken into consideration. If, for example, for every ten times the always-eager male approaches his mate, only once is she physiologically attuned, and psychologically inclined, to receive his 'addresses,' we should call her cold or coy on the predominant evidence of the nine occasions though she may be extremely ardent on the tenth. I am disposed to believe that such evidence as we have is in favour of this view. But it may here be asked: If this be so, wherein lies the biological value of the persistent ardour of the male? In reply it may be said that, on the basis of general considerations, it seems advantageous that *one* of the partners should be ready at all times during the pairing season. For if each were ready only on some tenth occasion, pairing would depend on the chance coincidence of this tenth occasion in both sexes. But, it may still be said, if the so-called preference of the female mainly depends on her physiological state at the time, will she not then pair with *any* eager male that chances to be at hand? When the pairing hunger is insistent, will it not be satisfied by union with *this* mate as readily as by *that*? Perhaps in many cases this is so. The evidence alone can decide. In seasonally monogamous species, only her already 'chosen' mate is at hand—though if he be killed a successor is soon in evidence. But, to follow up the hunger analogy, does it follow that because hunger is a condition of taking food, the hungry animal will eat *anything*? Surely notwithstanding the presence of hunger, there may be, and commonly is, some 'preference' for *this* over *that*. May we not say that although a certain internal state (disposition) is a factor in every instinctive response, there is also required an adequate incentive to that response? Is it therefore unreasonable to suppose that even when the wave of pairing sex-hunger comes over the female, the 'display' of *this* male may, in some cases at least, afford an adequate expression, while the 'advances' of *that* male do not? If this supposition be justified by the evidence, then, in such cases, there is sexual selection. We must however remember that, if there be a process of sexual selection in intimate relation to other forms of natural selection, the outcome of its continuance for many generations would be to keep the expressional display of the male up to the level, or within a definite range, of adequacy. We do not meet with many failures under other forms of natural selection, just because such failures are so steadily eliminated. Ought we to expect to meet with many cases of lack of adequacy to afford the adequate impression for eliciting the pairing response? Their very absence may be a sign of the stringency of a selective process.

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[1] p. 330. [2] p. 561. [3] p. 342.

12.

I quite realise that some objection may be raised to my use of the word 'expression.' Why limit it to that which is correlative to impression? Well, why not, for the purpose in hand, if it serves to elucidate a point of view? That point of view is that what I have called impression is psychical in its nature, a way *in* which the bird or other organism is conscious, in the broad sense of this word; and that if expression is (under definition) correlative to impression, then there is a valid sense in which it may be said that the genesis of expression depends on—or, if it be preferred, has been evolved for the sake of—the impression it produces. If this be so, the importance of psychical selection receives due emphasis.

Part of my aim has been to show the intimate connexion of biological and psychological values though the latter are, in the cases discussed, at an early stage of their development. I wish to protest against an undue sundering of natural and psychical selection. Why should not both be regarded as fundamentally natural, our object being to correlate diverse aspects of that which is found in the given nature of one reality? This does not of course preclude the drawing of distinctions. In psychical selection there is preferential appeal of the best expression; under other forms of natural selection there is differential weeding out, the incidence of which falls initially on the worst individuals—those which are bionomically deficient. Psychical selection works from above downwards; natural elimination works from below upwards. But the net result in either case is the preservation of efficiency.

Apart from this it is, I think, only under a misapprehension that sexual selection is regarded as an alternative to natural selection. It is in truth a differentiated form of natural selection, dependent, according to Darwin on exclusive relation to reproduction. Darwin does, indeed, contrast sexual and natural selection, qualifying the latter, in one passage[1] by speaking of "ordinary or natural selection." But one is not, I think, departing from the spirit of his teaching, if one speaks of sexual selection as a differentiated form of natural selection. Let us assume, then, that sexual selection is analytically distinguishable from other forms of natural selection in and through its exclusive relation to reproduction; and that under the heading of sexual selection two sections may be distinguished; (*a*) that which includes those modes of elimination that do *not* involve what I have called the relation of expression to impression; (*b*) that which includes those modes of selection which *do* involve, in some form, this relation. And let us go a step further, and distinguish, under (*b*), two sub-sections; (*α*) that which includes those modes of expression and impression that are only indirectly related to the pairing act, which we may speak of, in monogamous species, as securing a mate; (β) that which includes those that are *directly* related to the pairing act. We then have the following scheme under which we may conveniently classify the observed phenomena.

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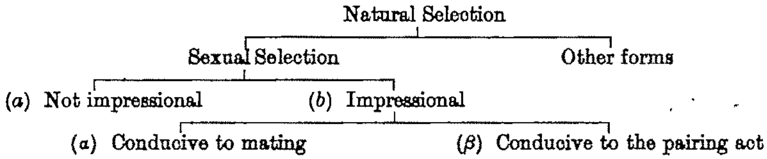
[1] p. 349.



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THE NATURE OF VERSE.

By E. W. SCRIPTURE.

I. *The controversy originating this inquiry. 'Quantity verse' and 'stress verse'* (pp. 225, 226).
II. *Apparatus employed and analysis of the record obtained. 'Enunciation verse' and 'melody verse'* (pp. 226–231).
III. *Conclusions as regards the controversy. Their general validity* (pp. 231–233).
IV. *Verse as a product of human activity* (p. 233).
V. *Fallacy of the 'metric' theory of versification* (pp. 233–235).

I.

In a dispute which arose between two professors of Greek at Harvard and Yale Universities concerning the nature of Greek verse, they ultimately had recourse to their own experiences in speaking English verse. The former professor maintained that anybody could hear that English verse consisted of long and short syllables, whereas the latter protested that anybody with ears knew at once that it consisted of loud and weak ones. The difference between their views may be illustrated in a somewhat exaggerated form as follows. The Harvard professor would say that the line

Somebody said that it couldn't be done

really sounded somewhat like this:

So-o-ombody sa-a-aid that it co-o-ouldn't be do-o-one.

Whereas the Yale professor would say it was like this:

Somebody said that it couldn't be done.

In the first case four of the syllables are made longer, in the second they are made louder. The first is called 'quantity verse,' or verse of long and short syllables. The second is called 'stress verse,' or verse of strong and weak syllables.

Neither the two professors nor their audiences at various philological meetings came to any agreement. Finally the Yale professor appealed to the Psychological Laboratory to devise some apparatus that would settle the controversy. This was the starting point of certain investigations on speech which have continued ever since. The methods first used were very elaborate and expensive, but it is now possible to register verse in such a way that the question of its nature can be answered with certainty and accuracy without great labour.

II.

The apparatus now used is shown in Pl. I, Fig. 1. The person speaks into a comfortable mouthpiece. The vibrations and puffs of air from the mouth pass down the wide tube to the recorder at the end. This recorder is of the form devised by the Abbé Rousselot of the Collège de France. Instead of the rubber membrane I use one of oiled cloth that responds quickly to every impulse of air and has no vibration of its own. This membrane has a latent time so small as to be negligible and is 'dead beat' like a galvanometer with damping. The movements of the membrane are enlarged by a light lever and registered on a surface of smoked paper around a revolving drum. After the sheet of paper has been filled, it is removed, passed through varnish and dried. It can then be studied at leisure. Before or after a record a tuning fork is applied to the drum, drawing a wavy line in which each wave indicates $\frac{1}{100}$ of a second: measurements of time to the thousandth of a second can thus be made. This particular drum has its speed fixed so that it is necessary only to test it from time to time.

An inscription of "Somebody said that it couldn't be done" is reproduced in Pl. I, Fig. 2. The first thing to be noticed is that it is a continuous movement. This spoken verse is a smooth gesture without breaks. It does not resemble in any way the printed verse consisting of a series of independent types separated by spaces. In the typed line there are six large spaces between what are called separate words, and a number of smaller spaces between what are called separate letters. In this particular inscription there are no spaces of any kind. This does not mean that spoken verse may not contain pauses. Pauses do occur with the greatest frequency as part of the expression; every pause of even a hundredth of a second is clearly indicated in an inscription. On this particular occasion there was no pause.

Similar sounds give similar inscriptions. Those that have an emission of air, such as *s*, raise the line of the record. Those that have a complete stoppage of the breath, such as the usual *b*, register as a line on the lowest level. Those that have a sharp explosion, such as the usual *b*, produce a sharp upward jerk of the line. Experience with inscriptions makes it easy to pick out the parts belonging to the different sounds.

The inscription in Fig. 2 begins with a rising line for the sound *s* which consists of a current of air forced through a small opening between the tongue and the palate. A somewhat similar raised line is seen for the *s* of *said*.

The small waves are registrations of the vibrations of the voice. They are larger for the vowels, somewhat fainter for the *m* and the first *n* and still fainter for *b* and *d*. They are very large for the final *n*. To avoid misunderstanding due to spelling, it must be remembered that the *e* of *some* is a superfluous letter for which there is no sound. The same is true of the *e* of *done*; the sentence ends with the sound of *n*. The *th* of *that* is not a double sound but a single one for which modern English possesses no letter because it has thrown away the old Anglo-Saxon one.

A fall of the line indicates that the current of air is being cut off. In the first *b* the line falls slowly; this shows that the lips close gradually in making this sound. When they open, the air escapes as a puff or explosion and the line is sharply blown up. This *b* is not a typical carefully made *b*; the line should have fallen quickly and remained on the base level till the upward jerk began. The first *d* is even more carelessly made; the tongue cuts off the breath gradually and lets it out again with practically no explosion. The last *b* and the three other cases of *d* are somewhat better made, but the only really careful one is the *d* of *done*. The *d* of *said* is followed by a rush of air for *th*; it does not explode separately because the tongue simply changes its position in passing from the one sound to the other. The *t* at the end of *that* is very carelessly made. Moreover it has small waves that show the larynx to be vibrating, although in a properly made *t* this should not occur. The reason is clear. This *t* is between two vowels; the larynx does not take the trouble to stop, just as a motor omnibus slows up in the hope that the passenger will get on while it is in motion and thus save the omnibus the trouble and expense of stopping.

It is indeed quite evident that some of the sounds in this inscription are carefully made while others are spoken carelessly. The usual *s* registers as a smooth raised line. In the first *s* in Fig. 2 the line remains low for a while, showing that the tongue was so tightly pressed against the top of the mouth that the opening was a very narrow one. The upward jerk at the end, almost like an explosion, shows that the tongue was suddenly released. The second *s* shows some similarity to the first one. These two examples of *s* are made with excessive carefulness. Let us go over the consonants in succession. The first *s* is careful. The next *b* and *d* are careless. The *s* of *said* is careful. The following *th*, *t* and *t* are careless. The *c* is careful; so is the following *d*. The next *t* and *b* are run carelessly together. The last *d* is careful.

There are hence four regions in which the consonants are carefully made, alternating with three regions in which they are slurred over. Careful enunciation means more energy put into the organs of speech. This line therefore has four regions of increased energy of enunciation.

Energy of enunciation has nothing to do with loudness. Weak speech may be very carefully enunciated; loud speech may be quite careless.

We have thus at the outset discovered an entirely new principle of rhythm; we are quite justified in saying that this verse is an 'enunciation verse.'

It will be noticed that the line of the inscription rises higher in the regions around *said*, *could-* and *-ne*. There are thus at least three regions of greater intensity or stress separated by regions of weakness. This verse is quite evidently a stress or intensity verse. The Yale professor was therefore quite right.

Some of the sounds in Fig. 2 look longer than others. To get the lengths accurately the record sheet is placed under a microscope with a measuring scale. The speed at which the drum revolves is known. The measurements by the microscope can therefore be turned into time. For example, if the measurement on the record sheet shows that a sound has a length of 22 mm., and if the drum has been revolving at a rate of 1 mm. = 0·0057 sec., then the duration of the sound is

$$0{\cdot}0057 \text{ sec.} \times 22 = 0{\cdot}1254 \text{ sec.}$$

Although one sound passes without break into the next one, an attempt has been made to assign limits between them. The limits are somewhat arbitrary, yet they enable us to get measurements of the approximate lengths of time occupied by the sounds. The durations, or lengths, of the single sounds are given in the duration chart in Fig. 3. This chart shows a long *n* at the end but no other very long sound. The *o* of *Some-* might be expected to be long and the *y* of *-body* to be short, but the inscription shows the *o* to be shorter than the *y*. Similarly the vowel *ou* of *could-* is shorter than the *n* that follows.

The conclusion must not be drawn that the verse does not consist of long and short sounds. It is quite true that it does not consist of syllables each containing a long vowel alternating with syllables each containing a short vowel. The duration chart shows, however, that there are four regions where the sounds are longer alternating with regions where they are shorter. The first region of long sounds includes *So-*; the second includes *-y sai-*; the third includes *couldn'-*; and the fourth the word *done.* The verse is evidently a quantity verse composed of regions of long sounds alternating with regions of short sounds. The Harvard professor also was quite right.

Each of the small waves in the inscription records one vibration of the voice tone from the larynx. A long wave is the record of a low tone, a short wave of a high one. The horizontal length of each wave is measured under the microscope and the pitch of the voice is calculated for each instant.

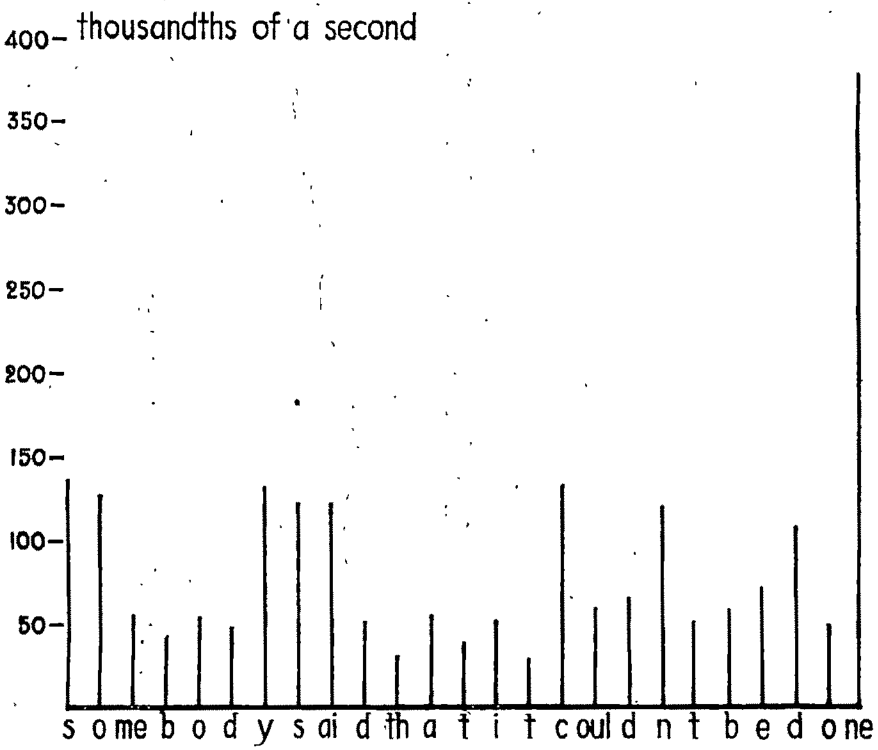

Fig. 3. Duration chart for the inscription in Fig. 2.

The inscription in Fig. 2 was taken at a speed of 1 mm. = 0·0057 sec. The first wave of the vowel *o* measures 1·4 mm. Its duration is therefore 0·0057 sec. × 1·4 = 0·00798 sec.

The pitch of a tone corresponds to the number of vibrations in one second. If a vibration lasts 0·01 sec. then the pitch will be 1 ÷ 0·01 = 100. For a vibration with the duration 0·00798 sec. the pitch will be

$$1 \div 0{\cdot}00798 = 125.$$

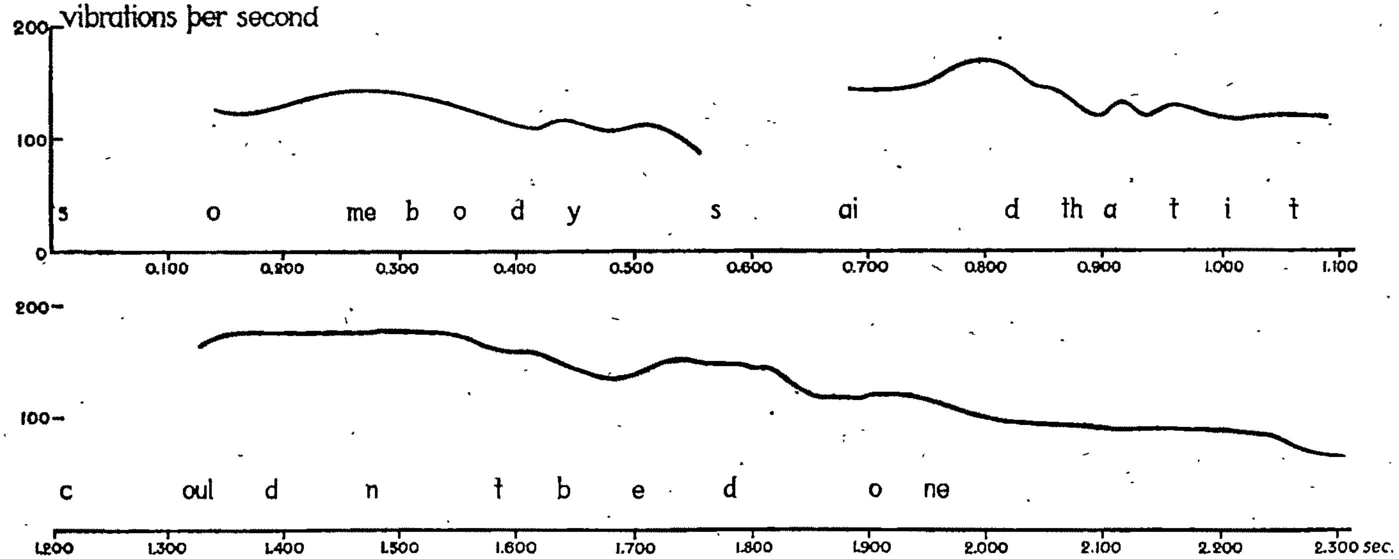

Fig. 4. Melody Plot for the inscription in Fig. 2.

When the pitch of the tone corresponding to each vibration has been calculated, the inscription is supposed to be laid along a horizontal line. Above each wave a dot is placed corresponding to the pitch. A line is drawn through the dots. The result for Fig. 2 is given in Fig. 4. This line indicates how the voice rises and falls during the sentence. It may be called the 'melody plot.'

The voice rises during *-ome*; it starts high and rises still higher in *-ai-*; it starts high and remains very high in *-ouldn'-*. Between these high regions there are regions of low pitch. A regular rhythmic effect is given by the alternations of high and low pitch. The verse is therefore a 'melody verse.'

III.

Each of the professors was quite right as far as he went. English verse is a quantity (long and short) verse such as Latin verse is supposed to have been. It is also a stress (loud and weak) verse such as Anglo-Saxon is supposed to have been. Neither of them thought that it might be a melody verse, as some persons have asserted that Greek verse was. An enunciation verse was something yet undreamed of.

Wherein they were both wrong was in the method they employed. Each appealed to the ear. But anybody with a prejudice can be made to hear anything; all that is needed is to create the proper expectation. Consequently the Yale professor was more nearly right; for ultimately he appealed to experimental methods.

The professors might be either right or wrong from another aspect. They assumed that all human beings are much alike, that the rhythmic instinct of the Greeks did not differ essentially from ours, and that therefore what is true of English verse might be true of Greek verse. Several problems are involved in this assumption. In the first place, are the principles found for the line of verse in Fig. 2 valid for English verse in general? In the next place, are they valid for the verse of other languages?

As regards the problem whether the above principles are generally valid for English verse, the following data are at hand at the present time. In the first place a study, just finished, of "To be, or not to be," gives exactly the same results as those above described. Studies—yet unfinished—of "When that Aprile with his showeres soote" and "Beowulf was braeme, blaed wide sprang" (spoken by a person of the present day) lead to the same results. A study of the sentences "Fred is sleepy," "*Fred* is sleepy," "Fred *is* sleepy" and "Fred is *sleepy*" shows that the emphatic words get their effect from one, two, three or all the elements of better enunciation, greater intensity, longer duration or higher pitch. The same conclusions have been drawn from studies of the inscriptions of the interrogative sentences: "Is Fred sleepy?" "*Is* Fred sleepy?" etc. and from the exclamatory sentences: "What a sleepy Fred!" "*What* a sleepy Fred!" etc. Not only that; the more emphatic parts of a simple sentence such as "Fred is sleepy" use one or more or all the four elements to distinguish them from the unemphatic parts. Numerous other English sentences have been studied in the same way with the same results. In fact, what has been stated above as the law of English verse is really the law of English speech in both prose and verse, namely—the stronger parts of a phrase or a sentence obtain their effect by the use of one or more of the four elements of better enunciation, greater loudness, longer duration or higher pitch.

How then does English verse differ from prose? Studies of inscriptions of "Thus I pass by and die" as prose and as verse show that in prose the waves of emphasis do not recur regularly whereas in verse they correspond to a regular rhythm such as the movements in walking or dancing. The discussion of the nature of prose must be reserved for some other occasion. It may be pointed out, however, that some so-called prose is really verse even if it is not divided up into lines. I have read through long passages in Shakespeare's plays, where the print indicates prose, without being able to pick out a sentence that was not as perfectly rhythmical as his verse. On the other hand, there are not a few poets wholly lacking in rhythmical sense whose 'poems' are really nothing more than unrhythmical prose cut up into pieces and printed in lines and stanzas so as to delude the public into believing them to be verse.

Do these principles of verse apply to all English people? There can be no doubt about the answer. All English people walk, bicycle, dance, and perform other rhythmic movements in about the same way. Some do the acts skilfully and some awkwardly, yet the principles are the same.

Do these principles apply to other languages? At present I have only the following data. A study of inscriptions of "Ein Fichtenbaum steht einsam im Norden auf kahler Höh'" spoken by thirteen persons from different parts of Germany showed that all of them used the same melody although they came from regions where the melody of ordinary speech differed greatly. Of course, this was the result of their education in speaking the language of their culture, namely, modern high German. No matter how differently they spoke at home, they all spoke nearly alike in the matter of literature. The law of variations in melody as an element of verse was the same for all. No observations were made on the other elements. We can anticipate, however, that the same principles apply to all verse for the reason that all human beings act on roughly the same lines. In one language the intensity element may be the most prominent, in another the duration element may play a more important rôle, etc.

For Latin verse the same principle must apply. We cannot doubt that the syllables were in general long and short as we were taught in school. But now we cannot doubt that the so-called long syllables were often also louder, more precisely enunciated and higher in pitch. It is quite impossible to believe that a Roman could, even if he tried, say the words "Arma virumque cano" all in exactly the same pitch, with exactly the same intensity and with exactly the same carefulness. An organ might produce tones in that way, but such a demand on a Roman would be quite inhuman.

IV.

Verse is a product of human activity and must conform to the laws of the psychology of action and conduct.

In the first place, spoken verse is a course of continuous action, not a series of disconnected jerks. Except where jerks are purposely introduced spoken verse is a smooth flow of sound.

In the second place, spoken verse is produced with a rhythm or beat effect. To the continuity this adds the effect of a regularly recurring impulse. Here again there is nothing jerky or interrupted. The beat effect is not like that of sudden taps separated by silences; no human activity is ever of this nature. The foot strikes the floor at regular moments in dancing, but these taps are only phases in a continuous movement. The manner in which the beat effect is produced in verse is explained above.

In the third place, the verse of a stanza or a poem is a continuous whole. There are beat effects of superior order that characterize the lines, and still superior ones that characterize the stanzas. The superior beat effects are produced by pauses, alliterations, rhymes, etc.

V.

We must rid ourselves of certain erroneous notions derived from printed verse. Verse existed long before type was invented.

With the spread of writing and printing there has arisen a new method of communicating verse, although a very imperfect one. To indicate the superior beat effects, the verse is printed in stanzas; the reader naturally makes a pause between them. A somewhat less superior beat effect is produced by printing the verse in lines; it is expected that a slight pause shall be made at the end of each line. The beat effect of blank verse is much injured if it is read without this slight pause. When rhyme is present, the line is usually made to end with the rhyming sound.

This is absolutely all there is to be said about *spoken* verse and its representation in print. Unfortunately students of verse have taken the *printed* verse as a basis and have built upon it a theory of versification called 'metric.' The characteristics of printed type have led them into a realm of fantastic error that has no counterpart in reality.

Printed verse consists of a series of letters. It is quite natural and easy to divide such a line of type into 'feet.' But, as we have seen, spoken verse cannot be so divided; there are no feet in spoken verse. The verse-typist would divide the speech of Fig. 2 as follows:

Somebody / said that it / couldn't be / done.

If any one attempted to say the words in that way, the result would not be verse at all. The absurdity is still more manifest when we note that the type-lover groups the words *that it* with *said* whereas they belong to *couldn't* and that he connects *be* with *couldn't* whereas it belongs at least equally to *done.*

The typist goes still further and divides the feet into smaller parts called syllables. He would mark them off thus:

Some-bod-y / said-that-it / could-n't-be / done.

Not the faintest trace of any such division can be found in Fig. 2. Moreover any such division into parts is quite contrary to the continuity of speech. If an attempt were made to speak the verse in Fig. 2 with division into syllables, it would cease to be verse or even true speech.

It is so easy to dissect printed type that we cannot wonder that the typist should divide words into their smallest elements. But we must object to his assumption that in so doing he has divided spoken language into its elements. When he speaks of single sounds, we must correct him into saying single letters or groups of letters. But his single letters do not represent single sounds; in continuous speech there are no single sounds.

With his notions of syllables and feet the typist proceeds to analyse printed verse. He erects whole systems of trochee, dactyl, iambus, anapæst, etc. A long syllable followed by a short one in a foot he calls a trochee. A long followed by two shorts is a dactyl. A short followed by a long is an iambus; two shorts followed by a long is an anapæst.

Much printed verse can be cut up on such principles. The printed verse corresponding to Fig. 2 he would cut up thus:

Sōmebŏdy̆ / saīd thăt ĭt / cōuldn't bĕ / dōne.

He would describe it as a dactylic verse of four feet with the last foot 'checked,' that is, incomplete.

But what can the typist do with the many poems constructed like Kipling's *Cities*? On the basis of long and short he would print

Thĕ Cītĭes ăre fūll ŏf prīde
Chāllĕngĭng ēach tŏ ēach.

How are these lines to be divided into feet? Is a line composed of iambics and anapæsts thus:

Thĕ Cīt/ĭes ăre fūll / ŏf prīde,

or is it composed of trochees and dactyls thus:

Chāllĕngĭng / ēach tŏ / ēach?

Neither system suits both lines. The typist may try to escape from the difficulty by saying that it is trochaic with an accessory syllable placed in front of the first line for purposes of effect or perhaps attributable to the poet's laziness or carelessness about his 'metre.' In fact, the typist has to build up a most complicated machinery of extra syllables, decapitations, resolutions, etc., in order to force English verse into any of his systems. It can never be made to fit smoothly or permanently. One recent writer claims great credit for showing that Shakespeare's blank verse is not iambic as is commonly supposed, but really trochaic with various accessories.

It ought to have been evident long ago that such a manner of treating verse must be quite wrong. The poet knows little or nothing about 'metric,' and the less he knows the better verse he writes. The public who reads or hears his verses likewise knows nothing about 'metric.' The poet feels a rhythmic swing to the verse as he speaks or writes it, and the public feels it as the poet intends. The study of speech inscriptions has shown in what this swing consists. Neither for the poet nor for the public, nor for the scientific analysis of the verse, does anything exist beyond what this study has shown. Metre has been defined by one of its advocates as the quantitative framework on which verse is constructed. More truthfully, metre can be defined as a purely artificial system of treating printed verse with which spoken verse has nothing to do. The whole system is reminiscent of the mediæval dialectics which must be dispelled before there can be any approach to the truth.

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EXPERIMENTS ON MEMORY AND AFFECTIVE TONE.

By W. WHATELY SMITH.

(*From the Cambridge Psychological Laboratory.*)

A. *Objects of the experiments* (pp. 236–237).
B. *Technique and procedure* (pp. 237–242).
    (i) *Methods of measuring affective value.*
    (ii) *Method of measuring memory value.*
C. *Results* (pp. 242–247).
D. *Conclusions* (pp. 247–250).

A. OBJECTS OF THE EXPERIMENTS.

THE experiments described below were undertaken with the object of investigating the influence of affective tone on the process of memory by as quantitative and unambiguous a method as possible.

It is not unusual to find in psychological textbooks statements to the effect that some such influence is exerted; but these statements are not, as a rule, supported by anything more scientifically precise than an appeal to common experience. It is suggested that we tend to remember 'pleasant' things and to forget 'unpleasant' ones, and it is pointed out that we are much more apt to forget tedious and disagreeable appointments, for example, than those which promise interest and pleasure. This may be true enough, but it is equally a matter of common experience that there are many intensely distressing experiences which we find it only too difficult to forget and which insistently obtrude themselves into our memories. Common experience is, in fact, as unsatisfactory and conflicting in this connexion as in most other cases. It is also too vague to be of much scientific value.

Evidence of a much more scientific nature has been brought forward by Freud and others of his school (cp. Freud's *Psychopathology of Everyday Life*). He has found that in many cases where certain special items of experience, such as proper names, numbers, etc., which one would expect to be remembered easily, are in fact forgotten or distorted, it is possible to trace an associative connexion between them and some distressing or disagreeable experience or 'idea' of a nature calculated to provoke mental conflict—they are, in fact, associated with 'complexes.'

From this he concludes that such cases of forgetfulness are special instances of the general process of 'repression.'

The reasons advanced in support of this view have, however, been criticized on various grounds; as, for instance, that they are in no way quantitative, that no *measurement* either of 'affective value' or 'memory value' is made in respect of the forgotten material; that the associations noted may themselves be to some considerable extent determined by the observer's interest in the subject and his views on the cause of forgetfulness; and that the material dealt with is of a somewhat highly specialised type.

I therefore felt that it would be worth while to investigate the question by a purely objective method from which these sources of error were eliminated.

B. TECHNIQUE AND PROCEDURE.

(i) I proceeded to attempt to measure, quantitatively, the amount of affective tone excited in different subjects by certain words, and also the ease with which these words were retained in memory, thereby obtaining data amenable to mathematical treatment.

The procedure was as follows:—

I applied to each of 50 subjects (36 men, 14 women) a word-association test consisting of 100 words.

The same list was used throughout except that after three subjects had been examined nine words were altered, in order slightly to increase the range of interests covered by the list. Otherwise the words used were substantially the same as those given by Dr Eder in the introduction to his translation of Jung's *Studies in Word Association*. The final list used is given in Table I (p. 238).

In addition to these, five 'practice' words (Hand, Chain, Face, Run, Egg) were given to each subject before starting on the main list. These were intended to enable the experimenter to make sure that the subject properly understood what was required of him, and to reduce, so far as possible, the effects on the early words of the main series of any initial embarrassment or agitation which the subject might feel.

Three different methods were used for detecting and estimating the affective tone evoked by the words:

I. The Psycho-galvanic reflex. (All subjects.)

II. The Reaction-time measured in fifths of seconds. (49 subjects.)

III. Jung's Reproduction test. (22 subjects.)

TABLE I.

1. Head
2. Green
3. Water
4. Sing
5. Dead
6. Long
7. Ship
8. Make
9. Woman
10. Friend
11. Cook
12. Ask
13. Cold
14. Stalk
15. Dance
16. Village
17. Pond
18. Sick
19. Proud
20. Bring
21. Ink
22. Angry
23. Needle
24. Swim
25. Go
26. Blue
27. Lamp
28. Carry
29. Bread
30. Rich
31. Tree
32. Jump
33. Pity
34. Yellow
35. Street
36. Bury
37. Salt
38. Dress
39. Habit
40. Pray
41. Money
42. Silly
43. Book
44. Despise
45. Finger
46. War
47. Bird
48. Walk
49. Paper
50. Wicked
51. Frog
52. Try
53. Hunger
54. White
55. Child
56. Speak
57. Pencil
58. Sad
59. Plum
60. Marry
61. Home
62. Nasty
63. Glass
64. Fight
65. Wine
66. Big
67. Carrot
68. Give
69. Doctor
70. Travel
71. Flower
72. Beat
73. Box
74. Old
75. Family
76. Wait
77. Cow
78. Name
79. Luck
80. Horse
81. Table
82. Work
83. Brother
84. Afraid
85. Love
86. Chair
87. Worry
88. Kiss
89. Motor
90. Clean
91. Bag
92. Choice
93. Bed
94. State
95. Happy
96. Shut
97. Wound
98. Evil
99. Divorce
100. Insult

Nothing need be said about the two last, but it may be of interest to give a few details about the first, which proved by far the most delicate and valuable test of affective tone.

The arrangement of apparatus was as follows:

A Wheatstone's bridge was used in conjunction with a D'Arsonval galvanometer (resistance 92 ohms) and the resistances and connexions were as shown diagrammatically in Fig. 1. The electrodes, E_1, E_2 consisted of zinc-plate discs about 2 inches in diameter, covered with wash-leather and soaked in concentrated solution of common salt. They were always applied to the dorsal and palmar surfaces of the subject's left hand, and the hand, electrodes, wool pads, etc., were strapped firmly to a wooden splint in order to ensure a firm and satisfactory contact. The galvanometer was used in conjunction with a lamp and a celluloid scale graduated in millimeters.

A 10-ohm shunt was connected across the terminals of the galvanometer and a two-volt cell applied to the bridge.

The general objects and nature of the experiment were briefly explained to each subject before starting operations; he was then seated in a comfortable easy chair in such a position that he could see neither the experimenter nor the apparatus, the electrodes were applied and the patient's resistance balanced on the bridge. The necessary particulars of name, age, etc., were then taken, the five practice words given and the bridge was rebalanced. The experiment proper then began and, unless the subject's resistance increased or decreased so much that the spot of light moved off the scale altogether, the bridge was not readjusted during the experiment. (In those cases when readjustment proved necessary suitable corrections have been made in the subsequent computations.)

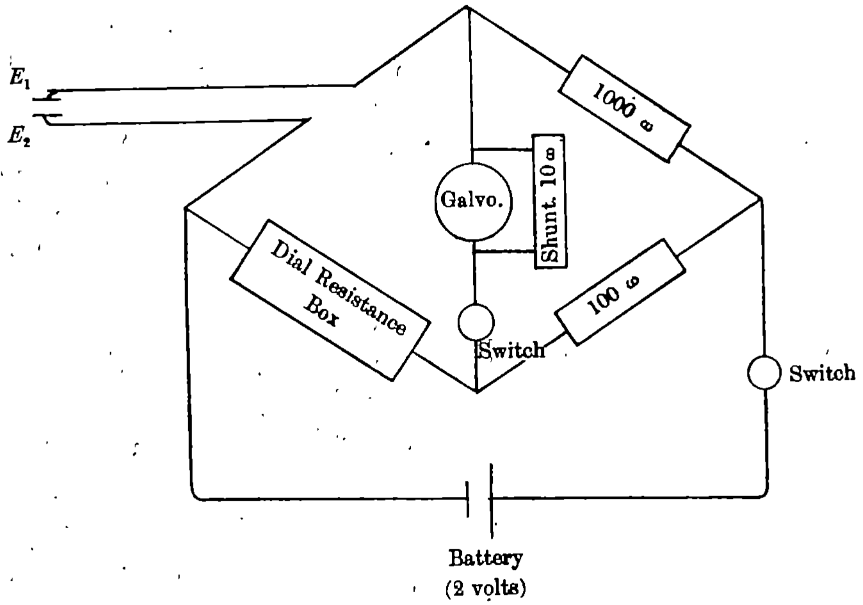

Fig. 1.

The data actually recorded for each reaction were:

(*a*) No. of the word in the list.

(*b*) Subject's response.

(*c*) Reaction-time: in $\frac{1}{5}$ seconds.

(*d*) Initial position of the spot of light on the scale at the beginning of its excursion.

(*e*) The position on the scale reached by the spot of light at the end of its excursion. (Galvanometric deflections were read to the nearest mm.)

(*f*) The difference between (*e*) and (*d*). (This was inserted later.)

(*g*) Whether the reproduction of the association was right or wrong.

A typical line of the record would thus read:

No.	Response	R.T.	From	To	Difference	Reproduction
9	Man	8	63	75	12	(Right)

In general the stimulus words were called out at approximately equal intervals, so far as was compatible with allowing the galvanometer to come substantially to rest between each excursion.

In the comparatively rare cases in which bodily movements or other causes prevented the movements of the galvanometer being read with reasonable precision, no entry was made under the 4th, 5th and 6th heads shown in the above record. These effects of bodily movements were easily recognisable, for the true reaction is always preceded by a latent period, whereas bodily movements which affect the mechanical conditions of contact produce their effect immediately. A very short experience enables the experimenter to distinguish infallibly between the two kinds of deflexion.

It was found that different subjects varied considerably in the reliability of the records it was possible to obtain from them. Some gave clear, regular, well-defined deflexions; others were much more erratic. In order to reduce, so far as possible, any accidental errors which might arise from this source, each set of observations was assigned a 'weight' according to the experimenter's estimate of their reliability. Observations of first-class reliability were weighted as 6, second-class as 5 and third-class as 4. Two exceptionally erratic subjects were weighted as 2 only. The weights were incorporated in the subsequent calculations in the usual way. This weighting was not applied to reaction times.

In order to make the deflexions given by different subjects as comparable as possible, I calculated the mean resistance of each subject by adding together the initial readings of the galvanometer for each reaction and dividing the sum by the number of reactions (normally 100). This gave the mean initial position of the galvanometer for each subject, from which, knowing the subject's resistance when the bridge was balanced, it was easy to ascertain—by direct calibration of the apparatus—the mean resistance of the subject during the experiment.

I then multiplied the deflexions of each subject by his mean resistance (in thousands of ohms) thus obtained. This eliminates variations due to subjects possessing different intrinsic resistances. (I hope sometime to give a justification of this statement in a special note devoted to the *technique* of the psycho-galvanic reflex.) Deflexions were also, of course, multiplied, for purposes of computation, by the appropriate 'weight' as already indicated.

Inasmuch as the resistance of most subjects decreased during the experiment, this procedure tends slightly to penalise the earlier words of the series as compared with the later words. But this does not affect the conclusions drawn from the experiments so far as memory is concerned and is, moreover, to some extent counteracted by the *torque* of the galvanometer suspension which tends to decrease the deflexion when the excursion begins at a high initial reading.

All the subjects were thoroughly normal and were drawn from the educated classes. Of the men 35 were between 19 and 30 years of age, while one was about 35: 15 of these were junior Naval officers, the remainder were University students. Of the women 12 were between 19 and 30 and 2 between 45 and 60.

(ii) In order to measure the 'memory value' of the words, I caused the subjects to learn by heart a selection of the stimulus words whose concomitant affective tone I had measured in the manner described above. These words were reproduced by the subjects at gradually increasing intervals and from the success of the reproductions a measure of their 'memory value' was obtained.

The procedure was as follows:

Two days after the experiment I sent to each subject a list of thirty words arranged in five rows of six words each[1], *e.g.*:

Long	Friend	Ask	Dance	Sick	Ink
Swim	Carry	Tree	Pity	Dress	Money
Finger	Paper	Frog	Hunger	Child	Plum
Marry	Wine	Carrot	Flower	Family	Cow
Name	Luck	Chair	Kiss	Choice	Insult

They were carefully instructed to commit these words to memory as mechanically as possible (*i.e.* not to invent any *memoria technica* for them), to destroy the list when they had learned it well enough to say it through once without a mistake, and to think as little about the words as possible when once they had been learned.

Reply postcards were then sent to each subject on the 5th, 9th, 14th, 21st and 31st days after the experiment with the request that the subject would write down as many of the words learned as he could then remember and would underline those over which he hesitated or which seemed to recur with difficulty. All subjects were warned not to 'rack their brains' or to make great efforts about the reproductions but rather to write down quickly and immediately as many words as came to them in the space of two or three minutes.

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[1] Association by meaning between successive words was sometimes inevitable, but its effects tell against, rather than in favour of, my conclusions.

In computing the results, words which were perfectly reproduced were given two marks for each reproduction, words underlined were given one mark and words not reproduced 0 marks.

Any word might, therefore, make a total score of any integer from 0 to 10. The total score actually made was taken as the memory value of the word concerned.

C. RESULTS.

In case it should be suspected that the magnitude of the galvanometer deflexion is not a quantitative indication of the amount of affective tone evoked by the corresponding stimulus word, I give here the mean deflexions, arranged in order of magnitude, which were obtained in response to the hundred stimulus words of my list (Table I) for the whole of the fifty subjects examined.

TABLE II.

Word	Defln	Word	Defln	Word	Defln	Word	Defln	Word	Defln
1. Kiss	72·8	21. Worry	33·0	41. Book	26·1	61. Cold	23·0	81. Bird	19·6
2. Love	59·5	22. Insult	32·5	42. Travel	25·9	62. Long	22·7	82. Bread	19·6
3. Marry	58·5	23. Friend	32·2	43. Sick	25·8	63. Go	22·6	83. Old	19·3
4. Divorce	50·8	24. Head	31·7	44. Bag	25·8	64. Cook	22·3	84. Cow	19·0
5. Name	48·7	25. Angry	31·5	45. Water	25·6	65. Yellow	22·2	85. Bring	19·0
6. Woman	40·3	26. Wine	30·9	46. Home	25·4	66. Chair	21·7	86. Clean	18·8
7. Wound	38·0	27. Luck	30·8	47. Big	25·3	67. Finger	21·5	87. Ink	18·7
8. Dance	37·4	28. Green	30·4	48. Bed	25·2	68. Sad	21·4	88. Sheet	18·6
9. Afraid	36·8	29. Ask	30·0	49. Silly	25·2	69. Tree	21·2	89. Table	18·5
10. Proud	36·7	30. Make	29·9	50. Brother	25·2	70. Needle	21·1	90. Work	18·3
11. Habit	36·6	31. Pity	29·7	51. Street	24·9	71. Blue	20·6	91. Carrot	18·2
12. Money	35·6	32. Choice	29·7	52. Beat	24·6	72. Ship	20·5	92. Bury*	18·0
13. Fight	35·0	33. Dress	28·5	53. Carry	24·5	73. Motor	20·4	93. Hunger	17·9
14. Child	35·0	34. Wicked	28·4	54. Wait	24·4	74. Frog	20·2	94. White	17·8
15. State	34·8	35. Dead	27·6	55. Speak	24·3	75. Walk	20·1	95. Glass	17·6
16. Despise	34·7	36. Sing	27·6	56. Box	23·9	76. Try	20·0	96. Give	16·7
17. War	34·1	37. Horse	27·1	57. Nasty	23·6	77. Plum	20·0	97. Flower	16·1
18. Family	33·6	38. Evil	27·0	58. Jump	23·5	78. Village	19·9	98. Pond	15·5
19. Happy	33·4	39. Doctor	26·9	59. Paper	23·2	79. Rich	19·9	99. Pencil	15·4
20. Pray	33·1	40. Stalk	26·2	60. Lamp	23·1	80. Salt	19·8	100. Swim	14·2

* Note: this word was frequently understood as 'Berry.'

The deflexions given are weighted means and may be taken to represent the reactions of a typical 'standard' subject of resistance 1000 ohms.

It will, I think, be generally admitted that the earlier words in this list are intrinsically more likely to arouse affective tone in normal persons than those which appear later.

Of the first six words, five are closely connected with sex-life and therefore likely to be of the utmost affective significance. The other —'name'—may derive some of its importance from its associations with the 'ego-complex' (some subjects reacted with their own name), but I am inclined to think that its effect is mainly due to the operation of sex-factors, for the word is obviously likely to arouse thoughts of a wife, husband, lover or other person of sexual interest to the subject.

The dominance of this group of six words is very clearly demonstrated if the deflexions are shown graphically in order of magnitude. There is a steady and almost uniform increase of the ordinate from 'swim' to 'wound' inclusive when it suddenly shoots up in a most marked manner.

The somewhat low value for 'woman' as compared with the other members of the sexual group is apparently due to the fact that it possesses a much higher emotional significance for men (mean deflexion = 44·6) than for women (mean deflexion = 33·5), which is what one would expect.

Satisfactory reproductions of words learned were obtained from 41 subjects (31 men, 10 women).

The words learned by these subjects were classified according to their memory value (0, 1, 2, etc., up to 10) and the weighted mean of the galvanometric deflexions was calculated for each class. The probable error of these weighted means was also computed. The results are shown in Table III.

TABLE III.

Words scoring	No. of words	Weighted mean G.D.	Probable error of mean
0	326	25·62	1·03
1	23	46·26	7·93
2	77	21·73	2·13
3	12	40·00	10·31
4	22	22·89	5·25
5	12	27·35	5·48
6	31	25·44	3·63
7	38	21·61	2·48
8	93	33·47	2·51
9	102	38·10	3·64
10	457	36·93	1·34

(*Note.* The chance that the excess of the mean G.D. for words scoring 10 over that for words scoring 0 is accidental is about ·00001. The chance that the excess of the mean G.D. for words scoring 0 over that for words scoring 2 is accidental is about ·115.)

These figures are not very informative as they stand, owing to their irregularity and the great differences in reliability between the different means.

Their significance can be better brought out by drawing a smooth curve of the form $y = ax^2 + bx + c$,

through the points

$$x = 0,\ y = 25{\cdot}62,$$
$$x = 1,\ y = 46{\cdot}26,$$
$$x = 2,\ y = 21{\cdot}73, \text{ etc.},$$

by the usual method of Least Squares, *weighting the values of 'y' inversely as their probable errors.*

This curve is $y = {\cdot}324x^2 + 1{\cdot}16x + 23{\cdot}49$. For convenience of calculation the middle of the memory value range is taken as origin: this also applies to the equations given later.

This yields the following values for the mean G.D.'s:

TABLE IV.

Words scoring	Mean G.D.
0	25·8
1	24·0
2	22·9
3	22·5
4	22·6
5	23·5
6	25·0
7	27·1
8	29·9
9	33·3
10	37·4

This result is shown graphically in Fig. 2. It is clear that the words best remembered have on the average a very much higher mean galvanometer deflexion (*i.e.* arouse on the average considerably more affective tone) than those which are soonest forgotten; and that the latter have on the average an appreciably higher affective value than words which are moderately well remembered[1].

It must be admitted that the large probable errors of some of the means are calculated to arouse suspicions as to the reliability of the curve. Elimination of the least reliable values would not, however, alter its general indications. Moreover, the three curves obtained are concordant and, in addition, I have recently investigated a number of other points arising out of this experiment and using the same material; the results have been uniformly harmonious to an extent which would be impossible if the general form of this curve were incorrect.

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[1] In the latter, a still higher affective value would have been observed but for the effects of dilution (see p. 248).

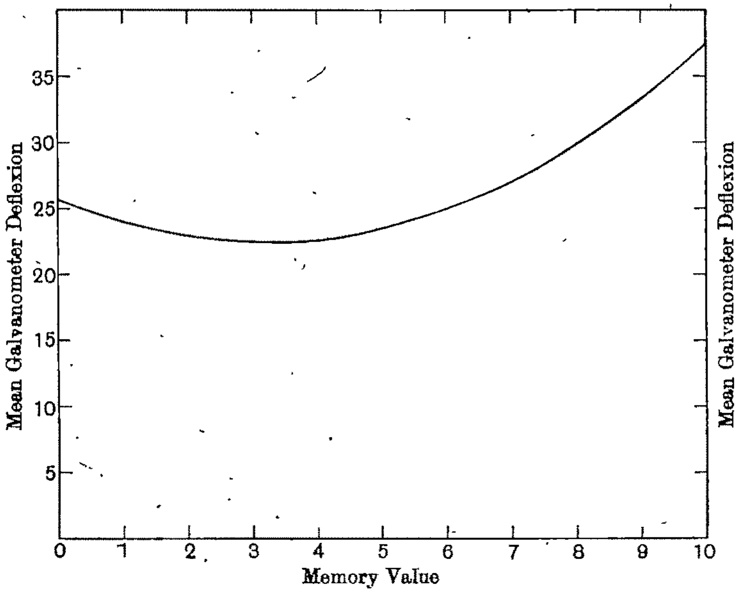

Fig. 2. Curve showing relation between Memory and Emotion as measured by the Galvanometer.

Similar treatment of the reaction times (40 subjects) gives the values shown in Table V.

TABLE V.

Words scoring	Mean R.T.	Probable error	Final values for representative curve
0	11·84	·25	11·99
1	11·13	·59	11·68
2	12·13	·49	11·44
3	11·58	·92	11·25
4	13·45	1·09	11·13
5	12·18	·84	11·06
6	11·16	·52	11·05
7	10·13	·34	11·11
8	13·31	·86	11·22
9	11·39	·48	11·41
10	11·67	·24	11·63

The equation to the representative curve is

$$y = \cdot 030x^2 - \cdot 036x + 11\cdot 06.$$

This curve is shown in Fig. 3.

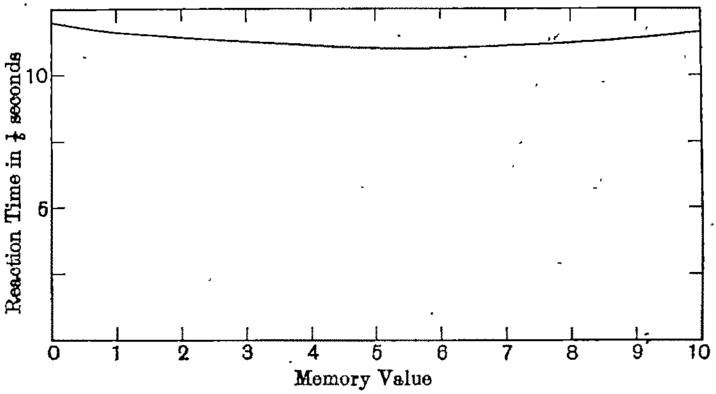

Fig. 3. Curve showing the relation between Memory and Emotion as measured by the Reaction Time.

Here again we find that the words best and worst remembered have a somewhat higher affective value than those moderately well remembered.

If we compute the percentage of failures in the reproduction test occurring among words scoring 0, 1, 2, etc. (20 subjects), and apply a similar method for obtaining a representative curve we get the figures given in Table VI.

TABLE VI.

Words scoring	Failures in reproduction (per cent.)	No. of words	Final values for representative curve (per cent.)
0	35·4	82	34·5
1	30·0	10	33·5
2	31·6	38	32·5
3	0·0	10	31·5
4	45·4	11	30·6
5	66·6	3	29·7
6	36·4	22	28·9
7	20·0	15	28·0
8	34·2	44	27·2
9	10·4	48	26·4
10	27·4	234	25·7

The equation to the representative curve is

$$y = \cdot 014x^2 - \cdot 88x + 29\cdot 73$$

and the curve is shown in Fig. 4.

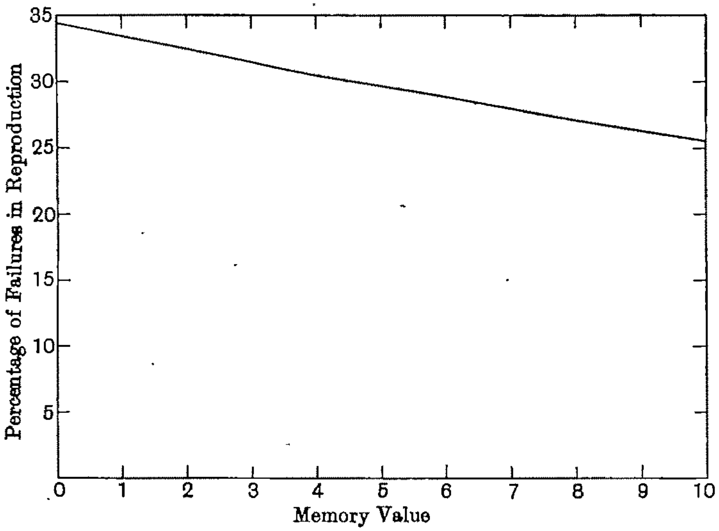

Fig. 4. Curve showing the relation between Memory and Emotion as indicated by faults in the Reproduction Test.

In this case it will be noticed that the curve is practically a straight line. There is a considerably greater percentage of failures in reproduction among words easily forgotten than among those well remembered. Practically speaking, the ease with which the word is remembered is inversely proportional to the tendency towards failure in the reproduction test.

D. CONCLUSIONS.

These results are entirely concordant with each other and clearly show two things:

First, that memory for words is influenced by affective tone; secondly, that, so far as the affective tone detected by the psycho-galvanic reflex is concerned, its influence may be exerted in two diametrically opposite directions; the fact that a given word evokes well-marked affective tone may lead to its being better remembered than a less intensely toned word, or may lead to its being forgotten more quickly. Affective tone as shown by the galvanometer deflexion should, therefore, be regarded as of two kinds, one of which facilitates, while the other impedes, the remembering of words which it accompanies.

On the other hand, the kind of affective tone which is shown by Jung's reproduction test is uni-directional in its effects and tends to impede the remembering of the words concerned.

The results obtained from reaction times are less marked than those given by the galvanometer deflexion or by the reproduction test, and at first sight appear to conform more closely to the former than the latter. That is to say, the affective tone shown by prolongation of reaction time appears almost equally likely to facilitate or to impede memory. But, for reasons which I shall give later (p. 249), I regard this appearance as misleading and consider that prolongation of reaction time is almost exclusively an indication of the kind of affective tone which tends to impede memory.

I do not propose here to consider the relation of the two kinds of affective tone mentioned above to the varieties commonly described as 'pleasant' and 'unpleasant.' This is a question which would take us very far and I think it wiser to adhere strictly to the necessary inferences from the experimental results.

I shall, therefore, speak of that variety of affective tone which facilitates remembering as 'positive' tone and of the opposite variety as 'negative' tone. To do this will imply no more than the bare fact already established that there is one kind of tone which aids the accession to consciousness of the ideas which it accompanies and another which impedes it. In very many cases, of course, pleasant and unpleasant will correspond with positive and negative respectively, but there are many exceptions to this and I feel that the former antithesis is of little value for the understanding of mental processes, because the effects of pleasantness and unpleasantness are not invariable.

The fact that the mean galvanometer deflexion for words scoring 0 is less than that for words scoring 10 (Table IV) is, in my opinion, due not to negatively toned words being less *intensely* toned than positively toned words and therefore giving a smaller deflexion, but to the fact that the words corresponding to this end of the curve contain a much larger proportion of 'indifferent' words (*i.e.* words of light tone, giving a small deflexion) than those corresponding to the other end. The words possessed of strong negative tone which are immediately forgotten (and so make a low memory score) are more copiously 'diluted,' so to speak, with indifferent words, which are gradually forgotten in the course of the experiment, than are those possessed of strong positive tone which are well remembered and so make a high memory score.

The effect of this will be to decrease the mean galvanometer deflexions (computed for a mixture of negatively toned and indifferent words), at the left-hand end of the curve corresponding to easily forgotten words. If we were to eliminate these 'indifferent' words, the curve would be far more nearly symmetrical than it actually is. Accordingly, I think it probable that the galvanometer is approximately equally responsive to positive and negative affective tone.

Similar considerations apply to the reaction time curve. The index to affective tone is, in this case, the prolongation of the reaction time and the effect of dilution with indifferent words is to depress the left-hand end of the curve. If this dilution were eliminated the curve would slope downward from left to right more steeply than it actually does. That is to say, prolongation of reaction time is, in general, a sign of negative tone.

The curve for the reproduction test shows, even as it stands, that failure in reproduction occurs mainly in the case of words easily forgotten —*i.e.* 'negatively toned' words—and here again this effect would be considerably increased if it were not for dilution by indifferent words.

This agrees with what is already known about the reproduction test, and the same comment applies, though less forcibly, to the preceding paragraph.

One conclusion of some importance which can be drawn from the form of the curve for the galvanometer deflexions is that positive affective tone and negative affective tone are equally definite and, so to speak, 'real' things. That is to say, positive tone does not consist merely in the absence of negative tone; nor does negative tone consist merely in the absence of positive tone. If either of these two latter alternatives were correct, the curve would not show a minimum value as it does; in the first case it would slope downward throughout from left to right, in the second from right to left.

This influence of affective tone on memory is relevant to the rate of forgetting. It is well known that the rate is more rapid for the period immediately following the learning of the material concerned than it is for later periods. I consider that this effect is largely due to qualitative and quantitative differences in affective tone between different parts of the material.

A number of words, for example, all of which were accompanied by equal and similar affective tone, would be forgotten according to some unknown rule determined by other than affective causes—possibly at a uniform rate,—whereas differences of affective tone would result in the negatively toned words being forgotten sooner and the positively toned ones later than would be indicated by the rule. This would result in the number of words forgotten per unit of time (*i.e.* the rate of forgetting) being greater in the earlier stages of the process and lower in the later stages than would be the case if affective factors were not operative. In other words, if we could discount the effect of affective tone, the curve connecting the quantity of material remembered with the time elapsed since learning it, would approximate more closely to a straight line than it actually does. It seems probable that considerable light may be thrown on the nature and relative importance of the factors involved in memory by further investigation of this point.

(*Manuscript received* 27 *August* 1920.)

REPORT ON THE SELECTION OF CHILDREN FOR HIGHER EDUCATION AT HAMBURG[1].

BY GODFREY H. THOMSON.

THE State of Hamburg decided to divide the elementary school above the fourth class; the bulk of the pupils at the age of ten, numbering about 20,000, continuing for another four years in the ordinary elementary course, the more capable, about 5 per cent. of the 20,000 being selected, with the aid of psychological tests, to proceed to a special five years' course. The latter was called the *F-Zug* or *Fremdsprach-Zug*, two foreign languages being learnt; the more elementary course was called the *D-Zug*, German (*deutsch*) being the only language.

990 children had to be selected. In the first place 1355 were chosen by the teachers, six boys and three girls from each school or twice these numbers in the case of certain double schools. The final selection was made on the combined basis of three factors. (*a*) The ordinary school report for each pupil; (*b*) a special psychological report, made by the class teacher who answered certain questions about each candidate on a special Observation Sheet; (*c*) a psychological group test, lasting for four hours,—a kind of examination in which, however, the tests were designed to prove not knowledge but ability.

The Psychological Laboratory of Hamburg prepared a full account of all the tests which had ever been standardised for such purposes as these; and their recommendations included such a large number of tests that the special 'coaching' of the children in all of them was quite impossible. The Laboratory is constantly adding to these.

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[1] *Hamburger Arbeiten zur Begabungsforschung.* Nr. 1, *Die Auslese befähigter Volkschüler in Hamburg*, von Rudolf Peter und William Stern. S. x + 157. 1919. Mk. 12. Nr. 2, *Untersuchungen über die Intelligenz von Kindern und Jugendlichen.* Herausgegeben von William Stern. S. iii + 167. 1919. Mk. 15.60. Nr. 3, *Methodensammlung zur Intelligenzprüfung von Kindern und Jugendlichen*, von William Stern und Otto Wiegmann. S. v + 256 1920. Mk. 24. Beihefte zur Zeitschrift für angewandte Psychologie. VI Folge. Leipzig: J. A. Barth.

The eight tests used at the examination, held at Easter 1918, were as follows:

(1) Definition Test: the four words 'booty,' 'uncle,' 'courage,' and 'envy' had to be defined. (2) Fable Test: the moral contained in two fables had to be given. (3) Criticism Test: the absurdities contained in a short story had to be pointed out. (4) Observation Test: long sentences were given once, and had to be written down from memory. This test was introduced to ascertain whether only odd words, mechanically remembered, or whether the substance of the whole, possibly in other words, could be reproduced. (5) Conjunction Completion Test: in a piece of prose the conjunctions were left out and these had to be filled in. (6) Three-word Test: out of three given words to make a sensible sentence. (7) Arranging Ideas: a number of words were given which had to be arranged in a sensible order. (8) Essay Test: two pictures were shown which gave two successive phases of an event and this event had to be recognised and described in a short essay.

The tests were given simultaneously at several centres. This involved the careful training of supervisors at each centre. The supervisors were members of the Psychological Laboratory Seminar Class, and exact instructions for their conduct of the tests had been printed and explained to them at several meetings beforehand. Indeed many of these supervisors had been engaged in compiling the tests which it was intended to give. The tests were finally conducted in 11 schools and 61 classrooms on two days. Each group was therefore of about 20 to 25 pupils, and had one supervisor and assistant.

The marking of the tests was very carefully planned, but cannot be described here. The system was one in which marks were given for errors rather than for excellences, and the children with the fewest marks were the winners, as far as these tests alone went. But the tests were not the only criterion, as was mentioned above.

The committee that finally decided which children were to be chosen consisted of five persons, namely a *Schulrat* (probably an experienced member of the Education Committee), a school-inspector, a male teacher, a woman teacher, and the director of the Psychological Laboratory. This committee had before it in the case of each child:

1. *The judgment of the teacher* whether the child was specially recommended or not.

2. *The ordinary school report* which in Hamburg takes the form of a number allotted to each child.

3. *The details of the year's work* of the child, giving particulars of

his marks in each subject of instruction for each of the past four years, particulars of attendance, etc.

4. *The result of the Intelligence Tests*, given as a number.

5. *The Observation Sheet*, on which the teacher had answered a number of questions relating to the child's various psychological qualities, such as memory, imagination, etc., and also inquiries about the leadership of the child in games, about his hobbies, and so on. This Observation Sheet itself would need several pages for a full description of its various features.

All the children whose marks in 1, 2, and 4 were uniformly good were taken without question. Similarly there was a set of children about whose rejection there was no doubt. The difficulties of the committee entered in deciding the border-line cases. Here the Observation Sheet became of remarkable assistance, If for instance the teacher noted thereon that the child worked as a rule very slowly but very thoroughly, this threw light on any failure to finish the task in the tests. In other cases it appeared from the Sheet that the school marks of the child were the result of industrious memorising rather than of real intelligence.

CRITICAL NOTICE.

BY CARVETH READ.

The Foundations of Character: being a study of the Tendencies of the Emotions and Sentiments, by ALEXANDER F. SHAND, M.A. London: Macmillan and Co. 2nd edition. 1920. pp. xxxvi + 578. 20*s*. net.

When this important book first appeared in 1914, only the briefest notices of current literature were ever given in this *Journal*, and unfortunately it was never noticed at all. We are very glad to see the issue of a new edition; chiefly, indeed, for the evidence thus supplied of a wide interest in the subject and of public appreciation of merit, but incidentally because it presents an opportunity to make up for former neglect.

It may be assumed that the reader is acquainted with Mr Shand's general conception of the organization of character—how impulses and instinctive actions are variously utilised in the systems of the emotions, and emotions coordinated in the service of the sentiments. An outline of this is given in the first part of the present volume; but to understand the full meaning of it in the constitution of character we must wait for the second volume, in which the laws of the sentiments are to be fully unfolded. It is not too much to say that an interested public is looking forward to the appearance of that work with flattering impatience.

The Foundations of Character is one of the most original works that British psychology has produced. It is full of matter of fact, experience and the witness of the greatest observers of human nature. The results of critical reflexion upon this material are summed up in a series of laws of the behaviour of the constitutive forces of human character, especially (in this volume) of the emotions. Shallow critics have complained that some of these laws are already commonly known; and that is true, but no valid animadversion; for in giving a systematic account all of them must be included. There is probably no reader to whom the greater part of them will not be new and enlightening. And the way in which they are arrived at is thoroughly scientific, namely, by beginning with approximate generalisations, and correcting them step by step in view of the exceptions, until a proposition that may be considered a law of the tendency of some character force has been provisionally stated. The author claims no more; he pretends to no finality. With a subject so complex and entangled there seems to be no other method; and one admires the patience, penetration, circumspection and resource with which it is carried out.

Still, although "the desire of perfection" (as someone said in expostulating with Napoleon) "is the greatest evil that afflicts mankind," yet it is so strong in a critic that he is sure to find some ground of complaint. The general conception of the problem in this book is biological, and if this had been strictly adhered to some positions might perhaps have been made clearer. For example, at p. 21 Mr Shand states the law of organization: (1) *Mental activity tends...to produce and sustain system and organization*; for, he says, the mind reflects the body. Should we not, therefore, add—*in relation to the environment*? Except in relation to the environment there is no such thing as character. Man, like every other animal (and Mr Shand never forgets his zoology) is adapted to live under certain conditions—social conditions being for him the most important; and although he has greater range of choice how to live—which expands with civilisation—yet there are always certain things he must do, or aim at, in order to live at all. Above all, he must plan for the future in order to live well; and, therefore, purposes, sentiments, desires, rule his conduct. In most cases a man does chiefly one thing; and, if he has been able to choose this freely, we may suppose that it represents better than any other would the strongest tendencies of his nature; whilst other things that he does at leisure, or as subsidiary tasks, represent his secondary tendencies—or with him who had little choice of principal task, the primary tendencies. In following out such purposes, he meets with various helps and hindrances, and various emotions become manifest, generally as reinforcing (or inhibiting) his actions in dealing with the situations arising from such helps or hindrances. A situation is constituted on the one hand by circumstances, and on the other hand by the man himself in such a disposition as he happens to be in at the time; and this situation excites his emotions. Situations, in their general setting, are very few (though in detail infinitely varied), and they frequently recur; and, therefore, the primitive emotions corresponding with them are also very few, as Mr Shand's analysis shows. Because the situations in their general setting are few, it is possible for man (or the higher brutes) to be so organized innately as to respond to them appropriately when they occur.

The emotions, then, are amongst the innate functions of a man's organization (with impulses and instincts) in relation to the environment; and perhaps it is only in this sense that they are *inter*organized; whereas Mr Shand appears to hold that they are innately organized in relation one to another (pp. 37–8); and this view leads to certain questionable modes of expression. He personifies the emotions, because each involves cognition, conation and feeling—the three essential elements of the mind as self; and this is still more plainly true of a sentiment (p. 64). The device is certainly convenient in exposition, and not merely for its literary effect. But taken along with the doctrine of an innate organization of the emotions, it results in some statements as to their operation one upon another which obscure the real causation. For example, Law (2), at p. 38, reads: *every primary impulse, whether it is independent or belongs to a primary emotion, is innately connected with the systems of fear, anger, joy and sorrow, in such a way that, when opposed, it tends to arouse anger, when satisfied joy; when frustrated sorrow, and when it anticipates frustration fear; these systems being similarly connected together.* Would it not be more accurate to say that a man is so organized that when any primary impulse is opposed, or satisfied, or frustrated, or threatened, the *situation* tends to arouse anger, or joy, or sorrow, or fear? The activity of the impulse is indeed necessary to complete the situations, but does not itself arouse the various emotions.

In several other laws in which one emotion is said to elicit another, it would be clearer to say that the change of situation elicits the other emotion. Emotions directly influence one another chiefly by mutual exclusion, when their physiological expressions are incompatible. Consider the following Law (91), p. 403: *Repugnance tends to exclude joy; and when its impulse is opposed, to evoke anger; and when it is frustrated, sorrow.* The first clause is strictly true; but the second and third clauses might begin—*and when its impulse is opposed, the situation tends to evoke anger*, etc. Similarly in the functioning of sentiments, if danger to a beloved object excites anger or fear, it is not love, but the situation of which love is a necessary factor that has these consequences. A sentiment (in this sense) establishes no interorganization amongst the emotions.

So much for the desire of perfection, that can only discover such formal flaws, and finds nothing to complain of in the essential method or in the wisdom of the book before us[1].

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[1] For want of a better place, a footnote may be utilised to draw attention to B. I, c. XI, *On the Relative Ethics of the Sentiments*: a remarkable chapter, which has not perhaps obtained the consideration it deserves.

The system of desires is discussed in B. III with great originality and penetration. Desires being excited by ideas of ends not immediately attainable, in our efforts to fulfil them success may seem more or less likely; and hence arise emotions peculiar to the state of desire, the 'prospective emotions' of Hope, Confidence, Anxiety, Disappointment, Despondency and Despair (p. 462). In the third chapter (p. 469) it is shown that these prospective emotions resemble (or how far they resemble) Joy, Fear and Sorrow. Such emotions arise (as Mr Shand expressly says) upon changes in the situation (or in our estimate of it), as more or less prosperous in view of the end. The system of a desire, therefore, is very much like the system of a sentiment. "Desire is a chief constituent of all human love" (p. 477); "there are some desires of such consequence that our whole nature supports them"—as when life is in danger; "the principal desire of every sentiment is supported by the whole strength of its system" (p. 500). These sayings seem to be true; and in the context it is not clearly shown how sentiments differ from the contained desires. But this lacuna has been filled in the third Appendix to the present edition; where we learn that a sentiment is richer and more enduring than its corresponding desire; for every desire comes to an end with its fulfilment, whilst there remains to the sentiment joy in the contemplation of the end. And the sentiment of love includes "the contemplative emotions—joy, admiration, wonder and sorrow apart from its desire" (p. 565); and hatred, of course, the opposite states of displeasure.

In this third Appendix Mr Shand also disposes of an objection raised by Prof. Boyce Gibson to Law (140): *Desire and Joy tend mutually to exclude each other* (p. 510). In the desire of Knowledge, it was urged, there may be a continual joy of progressive attainment. But "we must distinguish between a desire as it is felt in consciousness and the entire system of desire." Felt desires are incompatible with joy; but "when a desire in the second sense is active, as shown by the progressive accomplishment of its means, though not felt, there is no antagonism between desire and joy" (p. 559); hence when moved by a desire of knowledge, we enjoy the process of learning.

A reader may perhaps feel a doubt intruding whether, after all, the desires do not constitute the necessary skeleton of character; which might almost be conceived to live and work by itself without the flesh and blood of emotion and sentiment. It might be argued that the emotions arise merely to reinforce, or to give conspicuous expression to, tendencies and desires; and that sentiments result from the richness of

PUBLICATIONS RECENTLY RECEIVED

Mind and Work. By Dr Charles S. Myers. University of London Press. 1920. pp. xi + 204. 6*s*. net.

This book is an expansion of the industrial section of the author's *Present-day Applications of Psychology.* Written in untechnical language, it gives an extremely lucid exposition of the problems facing the psychologist who attempts to apply his science to industry. In successive chapters, general accounts are presented of Movement Study, Fatigue Study, Selection Study, Restriction of Output, Systems of Payment, and Industrial Unrest. Discussions of methods and principles are admirably interspersed with statements of results already achieved,—including some motion study results obtained in this country (pp. 6–11, 15).

Among the most interesting discussions are those on systems of payment and industrial unrest. "Under present conditions, a flat day rate for all is psychologically unsound, unless there be grades of day rate" (p. 150). As an alternative to payment by results, Dr Myers suggests that there should be in each occupation grades of workers, these grades being based upon individual differences in skill and capacity, and he gives an instance of such a system in operation (pp. 149–150).

Present industrial unrest is considered pathological. Partly due to the war, mental overstrain has produced on a large scale abnormal psychological conditions, such as excessive irritability, sensitiveness, and lack of self-confidence. Employer and employee have adopted defence mechanisms in blaming each other, and a general atmosphere of suspicion and unrest has thus become universal.

The book ends with a reference to the National Institute of Industrial Psychology, which has recently been established in London. The research this Institute is intended to promote should make important contributions to the problems outlined by Dr Myers.

A useful bibliography is appended to each chapter.

Psychology applied to Music Teaching. By Mrs J. Spencer Curwen. London: Curwen and Sons. 1920. Pp. xii + 304. 15*s*. net.

This book is dedicated to Professor John Adams and is based on Herbartianism. The sections devoted to anatomy and physiology are derived apparently from Carpenter and Huxley,—but with modifications! "At the base of the skull," we are told (p. 29), "in front of the medulla and including a part of it, there is a collection of ganglionic masses which are called the Sensory Ganglia, or, taking them together, the Sensorium." The "nerve-process leaves a path in the actual matter of the brain. Every time you repeat the passage the path becomes deeper and more definite.... After a while it does not go to the upper brain at all, but is switched off at another point, outside consciousness, at one of the lower centres" (pp. 33, 34). "When the object is no longer there you have no percept, but you have a mental image of it"

(p. 93). "We do the greater part of our thinking by means of images, more or less generalized" (p. 120).

Modern psychology may smile or sneer at such statements. But it must welcome in Mrs Curwen's book a sincere attempt to apply its general principles to the teaching of music. To persons so engaged, who have had no training in psychology, the book will be unquestionably valuable and suggestive. It is a pity, however, that Mrs Curwen has not laid more emphasis on educational experiment. So much has yet to be done in applying to different methods of teaching music the principles of scientific research.

Reports of the Industrial Fatigue Research Board. No. 8, *Some Observations on Bobbin Winding.* By S. WYATT and H. C. WESTON. Pp. 40. 1*s*. 6*d*. net. No. 9, *A Study of Output in Silk Weaving during the winter months.* By P. M. ELTON. Pp. 69. 2*s*. 6*d*. net. No. 10, *Preliminary Notes on the Boot and Shoe Industry.* By J. LOVEDAY and S. H. MUNRO. Pp. 32. 1*s*. 6*d*. London: H.M. Stationery Office. 1920.

In each of these Reports the psychologist will find material of interest to him. Messrs Wyatt and Weston draw attention to considerable individual differences in efficiency and their dependence on differences in ability, method and temperament; they urge careful selection of the workers, and individual, rather than group, training, in order to secure maximum efficiency. Mr Elton finds that "under artificial illumination production falls, even if electric light of sufficient intensity is provided.... Every unnecessary hour under artificial light means a direct loss of production and makes the task of the worker more difficult than it need be." He considers that "unbroken 4½-hour and 4¼-hour spells of employment are too long." Messrs Loveday and Munro find that the daily output "curve of the skilled workman is more regular and begins to fall later in the week than the curve of the operative whose skill is less, or who is ill-suited to his work." Mr Loveday describes an interesting experiment in the introduction of rest periods. Double presses were worked "with a team of three girls, each operative working 40 minutes in each hour and resting 20 minutes instead of with two girls working continuously throughout the day." The operatives were paid by day-rate *plus* a bonus on output. "The employees increased the output per machine by roughly 50 per cent." None of the girls desired to return to the former hours.

Psychoanalysis: its history, theory and practice. By ANDRÉ TRIDON. London: Kegan Paul, Trench, Trubner and Co. 1919. Pp. 272. 10*s*. 6*d*. net.

"What I have attempted to do," explains the author, "in the present volume is to sum up in a concise form the views of the greatest American and foreign analysts [the book is printed in the United States] which at present are scattered in hundreds of books, pamphlets and magazine articles. I have, whenever possible, presented their thought in their own words, through either direct quotation or condensation." The twenty-one chapters of the book cover a wide range of subjects, *e.g.* feminism and radicalism, the artistic temperament, crime and punishment, re-education and prophylaxis, the new ethics. A bibliography, containing over 200 references, and a glossary of psychoanalytic terms are appended.

Instinct and the Unconscious: a contribution to a biological theory of the psychoneuroses. By Dr W. H. R. RIVERS. Cambridge: University Press. 1920. Pp. viii + 252. 16*s.* net.

A critical notice of this book will appear in a subsequent number of the *Journal.*

Lehrbuch der Logik: auf positivistischer Grundlage mit Berücksichtigung der Geschichte der Logik. Von Dr TH. ZIEHEN. Bonn: Marcus und Weber. 1920. S. viii + 866.

Activism. By H. L. ENO. Princeton: University Press, and Oxford: University Press. 1920. Pp. 208. $1.50.

From the Unconscious to the Conscious. By GUSTAVE GELEY. Translated by S. DE BRATH. London: W. COLLINS and Co. 1920. Pp. xxviii + 328. 17*s.* 6*d.* net.

Phenomena of Materialisation: a contribution to the investigation of mediumistic teleplastics. By BARON VON SCHRENCK-NOTZING. Translated by Dr E. E. FOURNIER D'ALBE. London: Kegan Paul, Trench, Trubner and Co. 1920. Pp. xii + 340. 35*s.* net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

October 2, 1920.	The Bearing on Psychology of Experiments on the Sensitivity of Plants, by Sir Jagadiz Chunder Bose.
December 11, 1920.	Sensation and the Sensory Image, by H. Wildon Carr.
	The Psychological Basis of Liberty as a Political Idea, by Graham Wallas.

SECTIONAL MEETINGS.

(*a*) Education Section.

October 12, 1920.	Unconscious Determinants of Educational Attitudes, by S. S. Brierley.
November 9, 1920.	Problems occupying the present attention of Education in the U.S.A., by M. T. Whitley.
December 7, 1920.	Psycho-analysis and Parenthood, by J. C. Flügel.

(*c*) Medical Section.

October 20, 1920.	Some Biological Aspects of Sexual Repression, by J. C. Flügel.
November 24, 1920.	Study of a Severe Case of Obsessions, by James Young.

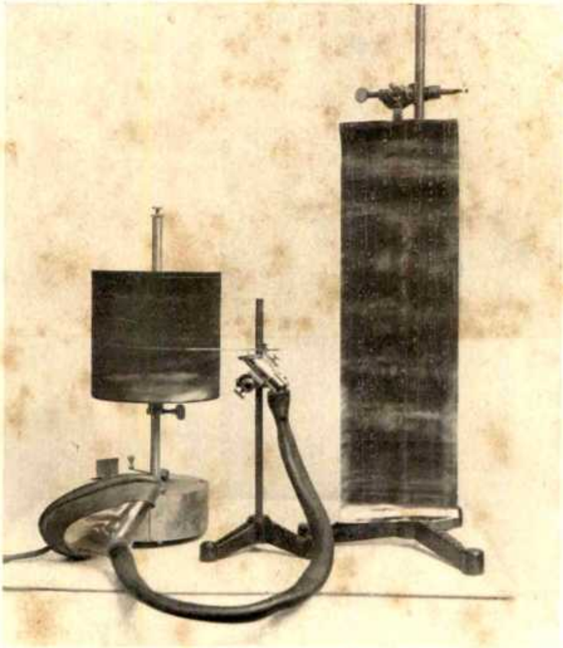

Fig. 1. Apparatus for making inscriptions of speech.

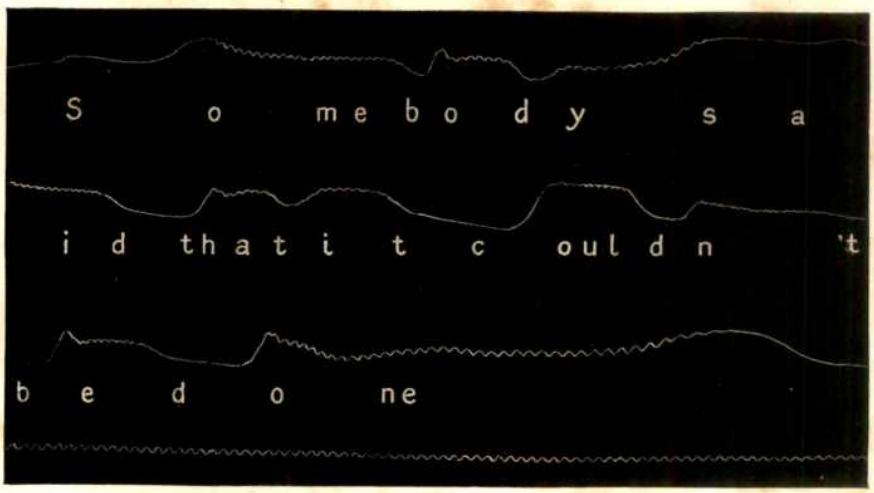

Fig 2. Inscription of "Somebody said that it couldn't be done."

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THE INFANTILE PSYCHE, WITH SPECIAL REFERENCE TO VISUAL PROJECTION.

BY DAVID FORSYTH.

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V. *Magic and the belief in spirits and demons* (pp. 271–273).
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VII. *Conclusion* (p. 276).

I.

IT has long been known that the process of 'thinking' conforms generally to one of two types. In some individuals it is dependent on a series of verbal ideas, in others on a series of visual pictures. Of the two, the visual type is the more primitive and is alone found in infants. In keeping with this is a phenomenon which never fails to surprise those who are beginning to acquire the *technique* of psycho-analysis, namely, that patients, when coming to narrate the experiences of early childhood, recall their infantile memories as visual pictures, detailed and vivid as happenings of yesterday. In much the same way, forgotten events of long ago are often reproduced in dreams pictorially. Visual hallucinations form yet another class of psychical phenomena closely related to the foregoing.

Of late years much attention has been given to the whole subject by psycho-analysts and attempts have been made by Freud and his followers to formulate the laws governing the production of dreams and hallucinations, the revival of visual memories, their association and their repression and substitution by other visual memories. In practically all these investigations the approach has been by the analysis of adult psychical structures whereby they are reduced to their infantile elements.

An alternative and, in some measure, complementary procedure is to study the phenomena as seen in early childhood. This latter method, however, presents obvious practical difficulties and, so far as I know, has not been adopted on a psycho-analytical basis. The account that follows is intended as a presentation of this other side of the question.

My interest was first definitely drawn to the matter by an incident, apparently trivial, which when reflected on, disclosed a wide field of attractive problems. It was this. One of my children, a boy of four years, who had just come from playing with his Noah's ark, was passing a half-lit landing *en route* for bed, and stopped to give his good-night kiss. He got so far as to put his arms round my neck when he suddenly drew back with a cry of surprise; and explained that as he was about to give his kiss he closed his eyes, whereupon he saw vividly before him the very animals which he thought he had left safely in the ark downstairs. It was evident from his further remarks that he in no way doubted the reality of this unexpected vision.

On a subsequent occasion the same child, when asked experimentally to shut his eyes and say if he saw anything, replied instantly, "Farmers raking hay," and readily identified the scene (and the farmer and farm hands) as an incident in his summer holiday more than two years before. Asked to repeat the experiment he now said he saw a picture from one of his books which he thereupon proceeded to bring from his bookshelf and quickly turned up a coloured illustration of hay-makers.

Both these memories came without any conscious effort and the child regarded them as nothing out of the ordinary. Nevertheless, the question arises as to why these particular images—of hay-making—presented themselves on that particular occasion, instead of any of the thousand-and-one other visual impressions which his mind had stored. The clue to this is to be found in the current circumstances. At the moment the boy was about to leave for morning school on one of the first bright days of early spring after the gloom of a long winter spent in London. His visual images indicated plainly enough where his feelings would have transported him. If this explanation is correct (and I do not doubt it), it is enough to show that visual memories are connected with emotions (inner excitations) and are revived in harmony with the prevalent feeling-tone at the moment.

Similar experiments with other children never failed to disclose these images, and in the case of one child—an intelligent boy of 10 years—a succession of no fewer than 201 separate pictures was described by him in a period of 40 minutes when the experiment was brought to a close as he complained of mental fatigue. In sum, these observations reveal a phenomenon which has been but little heeded but which would seem to be of universal occurrence. Moreover, as an every-day experience of individual children, it cannot fail to play an important part in their mental life. For these reasons a study of its various bearings promised to throw a light on psychic development at this period of life. Before the matter is carried further, however, some cognate theoretical considerations must be briefly stated.

II.

The special sense organs (eye, ear, etc.) are characterized by a high degree of sensitiveness, each to one special form of external stimulus (light, sound, etc.); and though for a short while after birth they are of small account, they come to exert far-reaching effects. The visual organ, which alone interests us here, transmits a centripetal wave of excitement which is registered in the mind as a memorative impression of the excitation. This visual memory becomes associated with inner (somatic) excitations, and can be subsequently activated from either of the two directions in which it has established excitatory connexions: (i) a repetition of the external stimulus will revive the original memory, which will bring with it some of the inner excitation previously associated with it, or (ii) the same inner excitation recurring, the memory will be activated even in the absence of an external stimulus. In the former event the nature of the affect which is roused by the later stimulus is not a matter of chance, but is already determined by the affective response to the original stimulus.

The alternative occurrence—activation of a memory by inner excitement—is responsible for and explains several psychical phenomena which have customarily been grouped together under the apposite name of 'imagination' (image-forming). In circumstances when this excitement has accumulated without relief and the tension has led to an overflow of energy to the psychical sphere, a visual memory (or memories) previously linked with this excitement at the time of an earlier experience, is revived; it is seen, so to speak, 'in the mind's eye,' or as a visual hallucination; and inasmuch as it is activated by inner excitation, its essential function is to serve as a substitutive or imaginative relief of this feeling, in lieu of real gratification; its purpose to realise an unfulfilled desire.

A further development of the same process is seen under the continued stress of ungratified excitement, when a series of hallucinatory images composes a day-dream or fantasy. Yet again, a similar awakening of memories by ungratified excitement is to be recognised in what is the most striking phenomenon of sleep, namely, the dream. This, like the hallucination and the fantasy, is an imagined gratification of feeling, and serves the purpose of wish fulfilment.

It is necessary, however, to recognise a peculiarity of visual (and auditory) memories as compared with impressions from erogenous zones. In the case of the latter, the stimulus (food, faeces, etc.) is necessarily in physical contact with the sensory surface; and when the memorative impression of the object providing the stimulus is vivified, the feeling associated with it is projected to the local site where the outer stimulus was originally applied (peripheral projection). But in the case of visual and auditory impressions, the object of which a mental trace has been formed lies at a distance, and the impression is projected, not to the eye or ear, but to the distant site whence the stimulus was derived. We recognise, therefore, a stage of 'exterior projection' of images.

III.

To return to the visual pictures seen by children. It is difficult for many adults who have left their childhood far behind to realise how important a part these hallucinations do, in fact, play in the daily life of children. Indeed, given the suitable condition of darkness, dim light, or even broad daylight provided the eyes are closed, children can hardly escape the experience of seeing visions. Not that they speak about them readily, and this for several reasons. Partly because they are so familiar with them that they take them for granted and so not worth talking about; partly because they are not encouraged to discuss them by their parents who often enough regard all such manifestations as unhealthy and consider that to attach any importance to them will foster a morbid mind—a tendency better met by ridicule than by sympathy; and partly because they like to keep their pleasant visions to themselves, and to forget the unpleasant ones as soon as possible. Nevertheless, they will speak freely enough to those who have their confidence, and no one who is responsible for the up-bringing of a child can afford, in the child's interests, to dispense with the help which these hallucinations offer—the agreeable ones give an insight into its joys and aspirations, the fearful ones into its apprehensions and terrors, and together they provide a running commentary on its psychical development.

In general, darkness is the necessary condition for 'seeing' these images (though the fact that merely closing the eyes serves the same purpose is proof that images are always flickering in the mind without being consciously recognised) and it is after going to bed, waking during the night, in dark rooms, stairways and passages that children mostly become aware of them. The forms they take are legion. One child, peering into a dark room, saw an angel dancing and beating time with its wings; at night-time an ugly old woman was seen grinning at the foot of the bed. In different circumstances different images are seen but every child is familiar with some which recur at short or long intervals, though even these are liable to cease appearing, their places being taken by others.

One of the most significant points in connexion with them all is that children project them externally. They see them, not in the mind's eye, but outside themselves, in the vicinity; and younger children at any rate never doubt that they are seeing real objects. Even fairly old children, though they may seek to explain them away as imaginary, can by no means altogether dispossess themselves of the feeling that they are real. In this they are like many adults who, under stress of feeling, fancy they see a ghost or the figure of a dead relative or hear an expected knock at the door[1]; reason may say that these are imaginary but the effect is as though they were real. The explanation of this is not far to seek. It is not, as most suppose, that the image provokes the emotion; on the contrary, the image is a memory which owes its revival to internal excitation, and the hallucination is not the cause but the result of the feeling. But the feeling is genuine and the assumption is made that the image is equally authentic.

Just as children, for several years, never suspect the reality of their night dreams, so they accept their hallucinations at the same valuation. It is possible, therefore, to form some notion of the kind of world in which every infant finds itself—a world in which the creations of the imagination are not in any way discriminated from the objective occurrences in the outer world. When, further, it is remembered with regard to hallucinations that emotional excitement may revive several memories simultaneously which may share little or nothing in common except that all of them have at some previous time established associations with this particular emotional tone, it will be realised how the most dissimilar images may be brought together in a single hallucination, and a situation represented which, in respect both to objects and actions, is impossible, fantastic, extravagant. Nevertheless, this is accepted without question as real. A mind which necessarily begins its development with an incompetency so complete, will, even years later, not only see nothing incongruous in fairy tales, myths and legends, but find itself accurately and sympathetically reflected in fabulous creations of all kinds.

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[1] And, of course, insane people may revert habitually to the childish belief in the reality of their projected images.

To the young child, therefore, the supernatural and the natural are one and the same, time and place impose no limits, physical laws do not operate. And it may safely be added that the younger the child the more completely do the two spheres of the real and the imaginary coalesce. The world to an infant is created and peopled in harmony with its changing feelings, to which, indeed, it merely serves to give expression.

This conclusion cannot be examined without its bearing on several matters of psychological interest becoming apparent. In three directions especially can it be usefully applied,—(i) psychic and objective reality: pleasure and reality principles, (ii) magic and the belief in spirits and demons, (iii) projection of feeling: rationalisation and magic.

IV.

Psychic reality and objective reality: pleasure and reality principles. The infantile psyche is totally unable to appreciate the essential difference between the objects which are seen in the outer world, and those which are the product of its own mechanism. Only in the course of years can this differentiation be made with certainty, and an intermediate stage must be passed through in which the two must often be confounded[1]. (Parenthetically, it would be instructive to know how often the look of open-eyed wonder on the features of an infant even of a few months is the result of a noticed contradiction as between the two spheres.) The question suggests itself as to which—the objective or the psychic—, when first the difference between them is becoming recognised, would present itself as true, which as fictitious. Familiarity must be the decisive factor. The earliest excitations are psychical and the approach to the objective can only be from the familiar to the strange. Moreover, hallucinated images are accepted as real and the emotions responsible for their revival are indisputably real. To the infant, therefore, the psychic world is the real world and the objective world is to be accounted for only in terms of psychic activities.

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[1] Most often, the psychic is mistaken for the objective, but the reverse is not uncommon. Thus, a child of 7 years, taken for the first time to the Victoria and Albert Museum and wandering through the (to him) vast galleries with their beautiful furniture, strange statuary, clothed wax-models and the echoing silence everywhere, exclaimed several times that he thought he was dreaming and would soon wake up and find it was all a dream.

But this, of course, is the contraposition of that taken in later years. Reality is then defined as 'actual being or existence' or as 'the quality of having an actual existence,' and the term is customarily employed in reference to the objective or physical world as perceived by the senses, and in contradistinction to 'imagination' by which I mean 'the power to reproduce an object of sense previously perceived.' According to this use of the terms, an hallucination, including both the visual image and the emotion that activates it, is imaginary and not real.

But the emotion is to be identified as nervous energy, itself a form of physical energy. Equally, all sensory stimuli coming from the outer world are forms of physical energy. No matter, therefore, whether it is a question of sensory stimulation or of emotional excitation, the manifestation is one of physical energy. Both are real in the sense of actual being, and the primary distinction between reality and imagination cannot be maintained. The relation between them might be usefully indicated by the terms objective reality and psychic reality, objective truth and psychic truth. And as, moreover, the phenomena of psychic reality are familiar experiences at an age when little or nothing can have been learned of the phenomena of objective reality, it must be concluded, if only on chronological grounds, that reality to an infant can only mean psychic reality and that the other kind has not yet become differentiated. The attitude is a reversed one: what is imaginary to us is real to the infant, and what is real to us is perceived by the infant only as part of psychic reality.

This brings us to a consideration of the 'reality principle' and the 'pleasure principle' which, formulated by Freud[1], have gained general acceptance as the two opposing principles shaping psychical development. Freud has recognised that the deeper, earlier and more primitive tendency is to seek pleasure (this includes avoiding pain), *i.e.* the relief of feeling, wish fulfilment; this is the only principle recognisable in infantile life. Later, it becomes opposed by the reality principle (which he regards as probably derived from it) which leads the individual towards adaptation to external environment, whether this process is immediately pleasant or not, *i.e.* towards 'adjustment to reality.' Subsequent to the infantile period both processes are in simultaneous operation, and the success with which the reality principle prevails over the other is largely the measure of the normal or the neurotic disposition of the adult; while only those of exceptional intellectual capacity are able to order their lives in accordance with the reality principle, since this implies severe renunciation of pleasure.

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[1] "Formulierungen über zwei Principien des psychischen Geschehens," *Jahrbuch der Psychoanalyse*, III. 1.

These principles can be usefully considered in relation to the projection of visual images in early life. Children accept their feelings and the hallucinations produced by them as real, and their behaviour is influenced accordingly, though neither feelings nor hallucinations have any objective existence. At the same time they lack experience of the external world —an experience which comes only by long use of their peripheral sense organs. The difficulty, therefore, with which they are confronted is that of reconciling the conflicting impressions received on the one hand from the external world through their peripheral senses, on the other from the inner psychical world; and of the two the latter bears the stamp of reality[1]. In other words, the pleasure principle represents the quest of psychic truth, the reality principle that of objective truth. The one concerns only the relief of feeling, the other the recognition of peripheral sensory impressions.

It would appear, therefore, that the reality principle is not genetically derived from the pleasure principle, but arises independently. Nevertheless, a factor common to the two must not be overlooked. Peripheral sensations imply excitation of the peripheral organs, whichever these may be, and this stimulation involves the relief of tension and is a source of pleasure. Sensory impressions, therefore, are themselves pleasurable in the first instance and the reality principle possesses at any rate one root which is hedonic.

It may be permitted here to indicate quite briefly two main lines of development, whether phylogenetic or ontogenetic, leading from the two kinds of reality. Where the pleasure principle dominates and psychic truth is accepted as the standard, interest passes to elemental psychical processes and thence to the supernatural and the spiritual; this is exemplified in the evolution of religion and of personal religious faith. Alternatively, with objective reality as the aim, chief importance is given to the facts of the physical and material world and thence to natural laws; along this line come science and an interest in science[2].

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[1] It is, of course, not only children who fail to accept the evidence of their senses when this conflicts with their feelings. Thus, an acute physician of my acquaintance whose only child developed a sore-throat which was obviously diphtheritic, was unable to accept the testimony he saw in her throat until four other physicians had been called in one after another and given the only possible diagnosis.

[2] The conflict between religion and science represents the difference between psychic and objective reality. Each gives credence only to the laws which operate in its own sphere. Each has truth indisputably on its side; each remains ignorant of the other kind of truth.

V.

Magic and the belief in spirits and demons. Another set of implications arises out of the fact that the child's world is created by its feelings and serves to give expression to them. As we have seen, visual projection is a phenomenon which results in a child seeing in its external environment objects which do not exist outside its mind and owe their psychical existence to its emotions at the time. With every change of feeling goes a change of projected visual images, and whatever the kind of feeling, the associated memories are projected externally. Daily experience of this can at first only support the belief against which for the time being no substantial evidence can be forthcoming—that it is its feelings which determine the conditions of the outer world, and therefore that its feelings are the central factor on which the outer world depends. In other words, the outer world changes in response to its own wants and desires, and this without any effort or action on its part. To wish is alone enough to bring about the change.

To illustrate this we may take the case of a very young infant—too young to have as yet acquired experience of the objective world and familiar only with its own psychical processes. Such an infant crying loudly with hunger will, under the stress of its desire, visualise and project an image of its mother's breast, perhaps of the mother herself. Soon the mother appears, suckles it and relieves its hunger. The infant is not to know that it was its cries that summoned the mother, nor can it as yet distinguish between the image and the objective mother[1]. The incident can contain nothing more for it than that, in response to its want, the mother appears and satisfies it. Experiences such as this furnish the ground for the belief that merely to wish effects changes in the outer world, *i.e.* that the world is subservient to its wishes.

Regarded from another point of view, this implies a belief in one's power of influencing the (unknown) forces of nature according to one's desires; and those who are familiar with Frazer's work on the rites and

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Of the two, psychic truth is the more primitive and expresses the deepest needs of the individual; objective truth is the more highly acquired and best serves in subduing the forces of nature in the interests of the individual. Neither can be disregarded and the strife between them cannot cease until both are recognised and directed to a common end.

[1] Against this it cannot even be urged that an apparition vanishes in a way which is impossible to a corporeal form. A babe who watches its mother disappear through an open doorway, or behind a screen and emerge on the other side, cannot possibly see in this anything but what it is familiar with in connexion with the coming and going of a mental image. Apparition or corporeal form, each dissolves as suddenly, completely and inexplicably as the other.

customs of savages and primitive races will recognise this belief as the foundation of the world-wide practice of magic[1]. Almost any of the abundant examples which Frazer has collected bears this out and it will be enough to quote a single instance—one which is given under the heading of 'Magical Control of the Sun.' "In ancient Egypt the king, as the representative of the Sun, walked solemnly round the walls of a temple in order to ensure that the Sun should perform his daily journey round the sky without the interruption of an eclipse or other mishap." The purpose of this ceremony is plainly to secure the fructifying influence of the sun in the Egyptian fields; behind this is the motive of hunger. The ceremony further discloses the belief that the sun can be controlled in harmony with the motive. Moreover, it need not be doubted that each day of unbroken sunshine was attributed by the Egyptians to the efficacy of the ritual performance.

It is not difficult to recognise the essential similarity, psychologically, between this regal circumambulation on behalf of the potentially hungry Egyptians and the visualisation of its mother by a hungry babe. In the latter case, feelings of hunger provoke the idea of food-breast-mother: in the former, similar feelings conjure up the idea of food-earth-sun. This calls forth in the one the imaginary mother, in the other the imaginary sun as represented by the king. In both the object of desire appears as subject to the control of human feeling. In both is implicit the assumption that the real mother and the real sun respectively present themselves in response to this need, *i.e.* that the objective world is controlled in the interests and at the behest of desire. But the case of the Egyptians is admittedly magic: the case of the infant, therefore, is of the same order.

It is only to be supposed that all children must pass through a belief in magic before they attain, by the help of their peripheral senses and guided by the reality principle, an understanding of the laws of nature, their immutability, permanence and independence of human wishes. Alike in the evolution of the human race and in the development of the individual child, magic is a direct consequence of the psychical function of the revival and projection of memories[2]. A further correspondence between phylogeny and ontogeny is to be noted. Evolutionally, the belief in magic gives place to religious belief; and religion in its earlier stages includes a belief in souls and in spirits and demons (these being later replaced by gods, and, later still, by a single god). Children, too, regularly pass through a stage, usually extending over several years, in which they believe in fairies, goblins, ogres, ghosts, etc. and show all the facility of primitive man in personifying inanimate objects[1]. The belief in souls arose, according to most writers, out of the experience of dreams in which absent or even dead persons appeared, *i.e.* out of the phenomenon of visual hallucinations. Similarly, spirits, demons and gods, which, like fairies, etc., can be nothing more than projected visual impressions, owe their place to the same psychical mechanism. In this connexion, recalling how children hallucinate most vividly in the dark, we may note as a suggestive fact that spirits and demons have most often been seen by night or in the dark recesses of trees, woods, caves, etc.—*i.e.* under conditions of light most favouring visual hallucinations[2].

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[1] See Sir J. G. Frazer's *The Magic Art and the Evolution of Kings*, 3rd ed. 1913, I. 312, etc. See also Freud's *Totem and Taboo Resemblances between the Psychic Lives of Savages and Neurotics*, Moffat, Yard and Co., New York, 1918, 138 *et seq.*

[2] In a remarkable paper, Ferenczi, approaching the subject from another direction, recognises in infantile psychology a "period of magic-hallucinatory omnipotence"—a stage corresponding with that dealt with above (see *Internat. Zeitschrift für Aertzliche Psychoanalyse*, I. no. 2).

VI.

Projection of feeling: rationalisation and magic. It is well known that savages hold their spirits and demons to be friendly or unfriendly (*i.e.* good or evil) which means that they are animated by love or hate[3]. The close psychical parallels that are everywhere found to exist between savages and children would suggest that the infantile mind similarly confers feelings on its own creations. The question therefore which presents itself is, Does the child mind endow hallucinated objects with feelings? If so, What determines the kind of feeling on each occasion?

Direct evidence bearing on this in the earliest years is, of course, hardly to be obtained, but so soon as children are able to describe their experiences of this kind no doubt can remain that, like savages, they believe the hallucinated objects to be animated by feelings towards themselves. In general these visions are either pleasant or frightening. To take one of the latter as an example—say, an ugly old woman seen in the dark near the bed—the child's explanation is sure to be that the sight of the woman suggests the thought that she is bent on harming him (*i.e.* is animated by hostile feelings) and that on this account he is frightened. But this, of course, is a complete inversion of the sequence of events which is, (1) fear begins to stir in the child, (2) this activates memories already associated with fear, (3) these impressions are projected, (4) the hallucination is now regarded as the cause of the fear, (5) the whole is capped by a false explanation, namely, that the figure is animated by hostile intentions.

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[1] Even the parts of their own bodies are regularly personified. Thus, a child of four years old was warned against swallowing gristle with his meat. Thereafter he came to regard his uvula as a little red man on guard in his throat to see that no gristle was allowed to pass.

[2] Pan himself dwelt in forests. Sir J. G. Frazer (*The Scapegoat*, 3rd ed. 1914) has collected accounts from all parts of the world of the occurrence of demons, and reading his pages the eye catches repeatedly such phrases as: at night, after dark, especially at night, in the mysterious darkness after sunset, at nightfall; while the habitats of the demons as given by different tribes and races include: thickets, hollow trees, great shady trees, bush, forest, woods especially, groves, jungle, forest glades, rocky places, ravines, depths of rivers, lakes or sea, caves, holes and dens of the earth, wells, steep mountain-sides (flat, featureless surfaces suitable to receive projected images), shadows of ruins, etc.

[3] In point of time, evil spirits are usually an earlier creation than benevolent ones. This is to be explained by the operation of the psychical mechanism of 'repression.'

In the case of a pleasant hallucination—*e.g.* the image of the mother projected by a hungry infant—it is permissible to infer a similar mechanism, once the child has gained some experience of the outer world and feels the need of accounting for its feelings. If it could speak, it would explain that the sight of the figure suggests the thought that the mother is coming to suckle it (*i.e.* is friendly) and that on this account its desire (= love) is stirred. Yet here, as before, the sequence is (1) desire for the mother, (2) revival of memories of previous occasions when the desire has been experienced, (3) projection of these memories, (4) the hallucination is accepted as the cause of the desire, (5) the assumption is made that the mother image is actuated by friendly feelings.

These five steps may be traced in all hallucinations, whether pleasant or the reverse, and the first part of our question is answered affirmatively: hallucinated objects are endowed with feelings by a child. In other words, projection of images is accompanied by 'projection of feeling.' This new psychical mechanism of projection of feeling is destined to play so large a part throughout later life, normal as well as pathological, that a few observations on its first occurrence is desirable.

That a projected image is held to be inspired by feeling is due, as we have seen, to the child's need of accounting for its own feeling. It is a false explanation which seems to explain and is accepted as explaining feeling (and behaviour) which in fact arise from another cause, of whatever kind, which is not recognised. It is justifying behaviour which is itself irrational by an explanation which passes as rational. It is, therefore, the earliest example of what Ernest Jones has called 'rationalisation'[1]. As manifested in adults—and it is a process as widespread as humanity itself and can be recognised in the reasons proffered by almost all people for their opinions and behaviour in matters great and small, from religion and politics down to the trifling affairs of everyday life—it represents the attempt to explain behaviour, which is based on unrecognised sources of feeling, on grounds which are intellectual and have reference to the outer world. It endeavours to credit to the reality principle what is in fact an indulgence of the pleasure principle.

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[1] "Rationalisation in Everyday Life," by Dr Ernest Jones, *Jl. of Abnormal Psychology*, III. no. 2.

Applying this to childhood, we see that the projection of feeling arises from a conflict between the inner needs and the outer reality, and is an attempt to bring the two into harmony. It can come into operation only after some experience of the peripheral senses has been obtained, and at a time when the child is not only already intimately familiar with perceptions originating within but accepts them as psychic truth—the only reality with which it is familiar. It aims, therefore, at subordinating the outer world to these inner perceptions. Its relation to magic is thus apparent.

It may further be said that rationalisation itself represents a modern persistence of the practice of magic. Its function is to pick out, reject, invent, twist or otherwise modify the evidence of the senses until it squares with the inner feelings. It, too, aims at subordinating the outer world to inner perceptions. In illustration of this we may take the case of a man who, on account of niggardliness, is unwilling to give a donation which has been asked of him; without acknowledging his motive he invents and offers in excuse half-a-dozen reasons which he believes to be the real ground of his refusal. Compare this rationalisation with *e.g.* the means taken by various primitive people to guard their cattle from evil spirits. In one country the peasants set thorny branches or sharp pitchforks against the cow sheds; in another they chalk crosses on the doors of their cattle stalls; elsewhere they stick twigs of birch in the dung heaps[1]. In each of these the wish to keep their property inviolate impels them to look for and use material objects which by suggesting protection give a feeling of security; therein lies their magic and they serve the same function as the rationalisations of the civilised man.

With regard to the kind of feeling that is projected on any occasion. We have seen in the case of the hungry infant that its desire for the mother becomes projected on to her as her desire for the child; that is, love is projected as love. But in the case of the ugly old woman fear is projected as hate. In each example rationalisation is reponsible for the projection, and the kind of feeling that is projected is that which would account for the internal feeling[1].

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[1] See "The Public Expulsion of Evils," pp. 160–2, in Sir J. G. Frazer's *Scapegoat* 3rd ed. 1914.

VII.

It was remarked earlier how very little attention has been paid to these visual phenomena; even those most directly concerned with questions of child psychology have passed them by practically unheeded. Yet the part they play in shaping and giving direction to the earliest stages of psychical development must rank high in importance. Not only are they constantly experienced by all children and therefore must fill a large part in their mental life, but the fact of their external projection must contribute more than anything else to enlarging and expanding the mental horizon to include what is external to the child. Moreover, the essential differences between a visual hallucination and a visual impression of an actual external object must confront a child with problems, confusions and contradictions, the understanding of which will do very much in helping forward its intelligence. Nevertheless, for a long time its attitude towards the outer world is based only on its experience of psychical reality, and is so unlike what later it will come to be that it is difficult for an adult to even picture it. It is a period when the imagination is unchecked by any contact with experience of objective reality, when the impossible happens as a matter of course, and the possible bears no distinctive feature by which it can be recognised. The world is not divided into real and imaginary; everything is magical, and there is no wish but what it comes true.

One last point. Are not the facts set out above enough to account in themselves for the fear which many children have of the dark? The relation between darkness and visual projection is intimate. It is specially in the dark that children, like savages, see apparitions, and these will be pleasant or terrifying. Timid children, *i.e.* those in whom runs a pretty steady undercurrent of fear, will be most liable to see terrifying visions, and therefore will most wish for night lights and companionship. Other children not specially subject to fear will have no such ground for shunning the dark. It seems hardly necessary to look further for an explanation.

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[1] The same process is at work often enough in adult everyday life. For instance, an individual who has taken a dislike to another is apt to assume that the other dislikes him, and may easily go so far as to account for his own antipathy as the result of the feeling which he thinks he has recognised in the other; and yet the second party may all the time be neutral or even friendly.

(*Manuscript received* 31 *October* 1920.)

A VINDICATION OF THE RESONANCE HYPOTHESIS OF AUDITION.

By H. HARTRIDGE.

(*From the Cambridge Physiological Laboratory.*)

1. *Evidence is given that a short silence is heard as a short noise* (pp. 277–282).
2. *The perception of high and low notes can be explained if the mass and tension (as well as length) of the vibrating elements of the cochlea are taken into account* (p. 283).
3. *There is no evidence that the number of vibrating elements of the basilar membrane is insufficient to account for the number of different pitches which the ear perceives* (pp. 283–284).
4. *The perception of the staccato ending of a note can be explained on the resonance hypothesis if the effects of successive contrast are taken into account* (pp. 284–286).
5. *The perception of the commencement of a note at full strength is explicable on the resonance hypothesis* (pp. 286–287).
6. *The resonance theory can explain how treble and bass notes sounding and terminating together are heard to begin and end simultaneously* (p. 287).
7. *Summary* (pp. 287–288).

1.

Perrett[1] has recently advanced a number of arguments against the Helmholtz hypothesis that the organ of Corti consists of structures which act as resonators. The more important of these arguments I propose to consider.

Helmholtz, in order to explain the perception of the separate notes which constitute trills in music, had to assume that, on the cessation of the sound, the resonating elements of the organ of Corti suffer a diminution of the amplitude of their movement to $\frac{1}{10}$ the initial value after 9·5 vibrations. With the voice pitched at 100 vibrations per second, therefore, an interval of ·1 of a second would be required for the intensity of the sensation to be reduced to $\frac{1}{10}$ and a still longer interval for it to be reduced to complete silence.

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[1] Perrett, *Some questions of phonetic theory*, 1919, ch. v, 32.

Perrett states that in the word 'utter' there is a short interval of silence between the two 't's' which can be shown by physical measurements to occupy about ·05 sec. (less than six vibrations with a note of 100 vibrations per second)[1], and that this interval is very much shorter than that required according to Helmholtz for the amplitude of the resonators of the organ of Corti to be reduced to $\frac{1}{10}$.

But the ear, which should not be able to detect this interval according to Helmholtz's estimate, is able to do so since it can differentiate 'utter' and 'udder' in which the gap between the two syllables is bridged by phonation.

Perrett therefore contends that the hypothesis of Helmholtz requires two incompatible conditions, for, in order to explain trills, a noticeable silence must not occur unless the silent interval is longer than the time period of ten vibrations, whereas to recognise the silent interval in 'utter' one corresponding in its length to less than six vibrations must be appreciated. But since Helmholtz's hypothesis cannot satisfy both these conditions at the same time it must give place to Wrightson's which can.

Against Perrett's conclusions two criticisms can be advanced: firstly, that the physical method of analysis is not sufficiently rapid or sensitive in its response to show the presence of vocal harmonics considerably higher in key than the fundamental note which may occur during the interval between the two syllables of 'utter.' Secondly, that if a silent interval between the syllables is the feature which causes the ear to differentiate 'utter' from 'udder,' then how is it that the ear can readily detect the difference between 'ut' and 'ud,' both of which are followed by silence?

Two questions, therefore, come up for consideration: firstly, can it be shown by experiment that a short silent interval can be appreciated by the ear, and secondly, if such is the case, can an explanation be found on the basis of Helmholtz's hypothesis?

With regard to the first question, it was found that one method, by means of which a short interval could be obtained, was with a syren consisting of a flat metal disc drilled with a circle of ninety-six equidistant holes, and capable of being rotated while a jet of compressed air was blown against the circle of holes. Preliminary tests having shown that a continuous musical note is obtained in this way, one or more consecutive holes were blocked with plasticine so as to produce an interval of silence of the required duration. Experiment showed that, with the ear within a foot or two of the disc, the interval caused by two consecutively blocked holes was clearly heard, and that the presence of one blocked hole could usually be clearly recognised. In fact it was found possible to gauge the speed of rotation of the disc by counting the number of times that the blocked hole made its presence heard per minute. Now with the disc rotating three times per second, 288 holes would pass in front of the jet per second and therefore one blocked hole would produce an interval of silence not greater than one 144th (·007) of a second. And with two consecutive holes blocked the interval cannot be greater than one 96th of a second (·01). These experiments, therefore, confirmed Perrett's conclusion that the ear can detect very brief intervals in the emission of sound vibrations, and indeed much briefer intervals than were given by him.

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[1] Perrett, *Nature*, May 16, 1918, 204.

In reference to the above syren experiment, an alternative explanation of the phenomenon might be advanced, namely, that it is not an interval of silence that is heard, but the beat of a tone. Now a beating tone would be produced by the summation of a note of 288 vibrations per second with another of 285 vibrations (since one hole in the disc is stopped), and therefore, beats would be heard occurring three times per second. This, however, is not what the ear perceives. There is not an alternate waxing and waning at the rate of three times per second, but a sharp click which occurs, once per revolution of the disc.

Now if, as Perrett said, the hypothesis of Helmholtz cannot explain the perception of an interval of silence of ·05 second, it would seem that this must apply with still greater emphasis to the shorter interval obtained by myself in the syren experiment. The following explanation, on the basis of Helmholtz's hypothesis, has occurred to me. When a regular series of vibrations falls on a set of resonators, at first a large number of them starts to oscillate in addition to the one which is in exact phase with the incident vibrations. But, one by one, they gradually get out of step with the incident vibration and are brought to rest, until finally only the one in tune with the vibrations is left in motion. If this gradual process of elimination occurs at the beginning of a series of vibrations, why should it not to some extent at all events occur if that vibration is interrupted for a time and then recommences? For this new phase would affect the resonators in the same way, to some extent, as it would have done if there had never been any previous phase, that is, for a short time afterwards other resonators are set in oscillation beside the one in tune with the vibration. The interval is perceived not because of the diminution in the amplitude of the swing of the chosen 'in tune' resonator, but by the feebler (and transient) swings of the 'out of tune' vibrators at the recommencement of the note.

This conclusion appeared to be sufficiently important (from the point of view of the Helmholtz hypothesis) to be worth experimental confirmation. The apparatus used is shown in Plate II (figs. 1—3), and will be seen to consist of a number of simple pendulums hung from a horizontal bar. This bar is given a to and fro motion by means of a rotating shaft, crank disc, connecting rod, etc., as illustrated. The shaft was rotated by an electric motor through suitable gearing. The motor was driven from accumulators so that its speed was moderately constant and could be adjusted between fairly close limits by means of a rheostat. Between the connecting rod and the pendulum mechanism was interposed a device by means of which the motion from the crank could be conveyed to the pendulums or be stopped for any required number of revolutions, but the arrangement was such that the connexion or disconnexion could only occur *at the dead centres*; a period of connexion or disconnexion could thus only consist of a certain number of half-rotations. When using this stopping and starting device, care was always taken that the crank was nearing the top of its throw, and thus each period of connexion or disconnexion consisted of a number of whole rotations. By this device, therefore, the motion conveyed to the pendulums could be started at dead centre, and after a certain number of complete vibrations stopped at dead centre again, and further, the interval of no motion (corresponding to silence) could be made to cover any required number of whole vibrations, while, on re-engaging the mechanism, the vibrations would recommence as before, at precisely the same rate and in precisely the same phase as those of the previous vibrations. By this means, therefore, the effects on a system of resonators of a musical note interrupted by periods of silence could be closely imitated.

In Fig. 1 is shown the effect of four complete vibrations starting from rest, the period of the vibrations being equal to that of the resonator marked *C*. It will be seen that beside the 'in tune' resonator *C*, other resonators on either side are in vibration, and these being out of tune with the 'in tune' resonator *C* will introduce that element of noise which is so noticeable a feature of the note emitted by a syren arranged to produce a series of groups of four consecutive sound waves followed by periods of silence. [In the above description the fact has not been overlooked that when the resonating system consists of a very large number of elements which differ from one another but slightly in period, a pure note will set not one element into oscillation alone, but a small group of

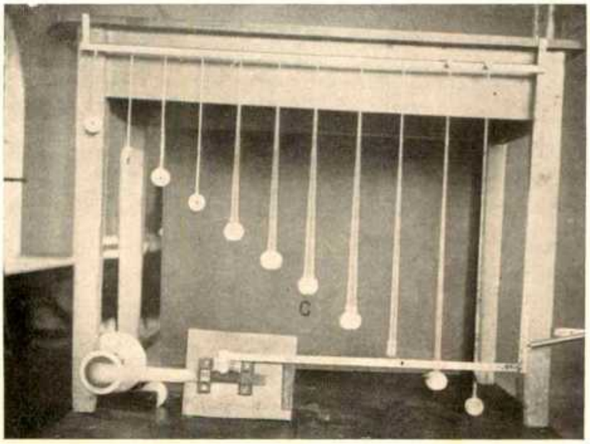

Fig. 1.

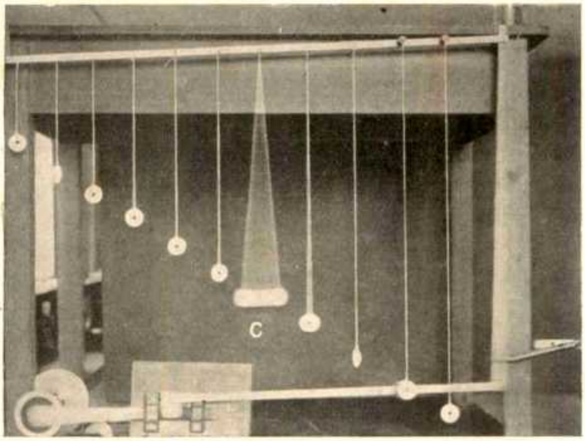

Fig. 2.

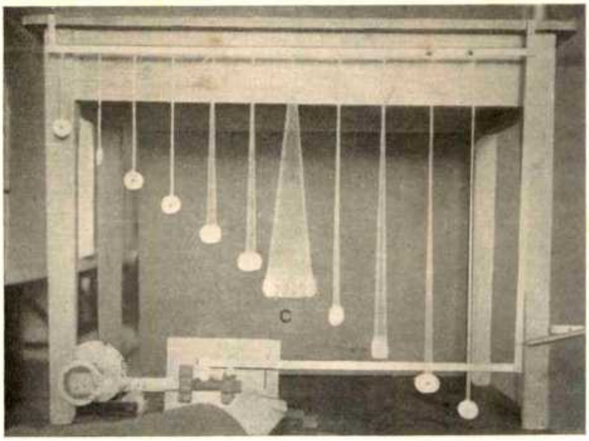

Fig. 3.

elements, the amplitudes of each one of which will obey some such mathematical curve as Maxwell's law of chance. In the above argument such a group of elements differing but slightly in pitch is, for simplicity of description, called the 'in tune' resonator.] If instead of four consecutive vibrations a large number be caused to act on the resonators, then all except the 'in tune' one get in turn out of step with the vibrations and come to rest. This condition is shown in Fig. 2. The small movements of this resonator may be due to the fact that the lever to which the pendulums are attached does not perform true simple harmonic motion because the connecting rod is not infinitely long (as it should be) and, therefore, at the top and bottom of the stroke, it makes a finite angle with the line joining the cross-head (the terms used here are taken from steam engine practice) and the power-shaft. Experiment shows that so long as the to and fro motion of the levers continues, so long does the 'in tune' resonator retain the maximum amplitude of its swing. If the motion of the levers be stopped for one or more whole periods and then started again, the 'in tune' resonator bridges the gap by continuing to swing with but slightly diminished amplitude (as Perrett stated that it would), but the resonators on either side also begin to swing again and continue to do so for a short time, not when the motion of the levers is stopped, but when it is started again (see Fig. 3). Applying these facts to the ear it would appear that it is not the silence which makes itself perceived by the resonators of the cochlea, but the recommencement of the sound waves, and further this recommencement does not show itself on the 'in tune' resonator but on the 'out of tune' ones. These, when they vibrate, introduce noise which begins at and continues for a short time after the recommencement of the sound. Now this conclusion can be tested, by means of the syren, by causing a brief noise to occur once per revolution, at the same time as a uniform note, and by comparing the sensations thus set up in the ear with those perceived when a silent interval occurs in a uniform note as in the first syren experiment. The arrangement used for producing the short noise was as follows: a single hole was drilled to one side of the row of holes previously used, and two compressed air jets were mounted so that one should blow as before on the row of 96 regularly spaced holes, while the other blew on the single hole whenever it came round. It was found that if the pressures of the air jets were suitably adjusted a change could be made from the original arrangement for giving a short silence (described earlier in the paper) to the plan just described for producing a short noise, without the ear being able to distinguish the one from the other. This would therefore seem to be clear proof that the ear does not perceive a short silence *as such*, but as the result of the noise produced when the sound waves recommence.

A further experiment was performed with the syren to demonstrate if possible the existence of a short noise at the commencement of a musical note. To a small loop in a length of fine silk thread was attached a ball of plasticine, which was pressed on to the end of the nozzle through which the compressed air was blown on to the 96 holes. With the syren disc rapidly rotating and with the compressed air turned on, the thread was suddenly jerked, causing the plasticine to leave the jet and thus allowing the air suddenly to impinge on the holes. In this way a uniform musical note was suddenly commenced from silence and the ear could distinctly hear the noise element at the commencement, which made the note commence as if with a 'c' or 'p' or some consonant between the two. Returning, therefore, to Perrett's original contention (that the organ of hearing as Helmholtz pictured it, *i.e.* as a system of resonators, would not be able to perceive the silence between the two 't's' of 'utter'), it would seem that if this interval of silence does indeed occur (and I have advanced above reasons against it), then it is the noise caused by the commencement of the second syllable and not the silence between the syllables that is in point of fact the mark of the interval. In the case of this word and certain other words that Perrett mentions (*e.g.* tick-tack) there are additional factors which affect the character of the recommencement of the second syllable. One of these is that during the vowel of the first syllable the air is being delivered by the lungs in a steady stream through the open air passages. When the consonant marks the end of the first syllable the escape of the air is prevented and therefore there is a rise of pressure of the air contained in the passages on the lung side of the obstruction (until finally such a value is reached that the lungs are brought to rest in spite of the continued contraction of the abdominal and thoracic muscles). Usually the second consonant (which marks the commencement of the second syllable) occurs long before this limiting pressure is reached, but not before it has suffered a considerable rise. On the commencement of the second syllable, therefore this pressure inside suddenly returns to its original value with an outward rush of air, and this adds considerably to the element of noise introduced as explained above by the recommencement of the sound vibrations with the beginning of the second syllable.

2.

Perrett (*loc. cit.* p. 10) states, "The basilar membrane being according to Professor Keith less than three times as wide in its termination in the apex as at its beginning in the base of the cochlea, the idea that it contains thousands of resonators accurately graded to respond to differences of one vibration per second or even less, becomes perhaps a little extravagant."

Now the formula for the rate of vibration of a stretched cord is

$$\text{Rate} = \frac{1}{2 \times \text{length}} \sqrt{\frac{\text{Tension}}{\mathit{MASS} \text{ per unit length}}}\,^{1}.$$

So that beside length, tension and mass are important features. These appear to have been forgotten by Perrett. The argument that the basilar membrane has nearly the same width at the one end as the other, in no way prevents the masses and tensions being such that the one end shall respond to notes of sixteen vibrations and the other end to notes of 50,000 vibrations per second. Further, whatever light histology may be able to throw on the lengths and masses of the various fibres, it would seem difficult to devise any means of measuring the tensions, so that if at some future date an anatomist should find all the fibres of the basilar membrane to have the same length and the same weight that alone would not prove anything either for or against Helmholtz's theory.

3.

Perrett, quoting Auerbach (*loc. cit.* p. 11), states that two successive notes differing by one quarter vibration per second can be distinguished in the best cases. "This would give over the whole audible range a total of 64,000 distinguishable pitches. But the number of vibrating elements in the cochlea is between 4000 and 8000, *i.e.* each fibre must be able to perceive eight to sixteen different pitches. This would appear to be an impossible task."

The validity of this reasoning depends entirely on the reliability of the two values which Perrett gives, *i.e.* that of the number of fibres and that of the number of pitches. With regard to the former McKendrick was satisfied that there were about 11,000 fibres in all[2]. With regard to the latter it should be pointed out that the differentiation of notes differing by one quarter vibration per second may have been due to

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[1] Poynting and Thomson, *Sound*, 1904, 88.

[2] McKendrick in Schäfer's *Text book of Physiology*, 1898, II, 1184.

the ear noticing a difference between the over-tones. Thus, assuming both notes to have over-tones equal to x times their own period, then a difference in these over-tones perceptible by the ear would lead to a differentiation of the notes themselves. Thus, supposing x to equal four, then the ear, by differentiating the over-tones differing by one vibration per second, would give the appearance of differentiating between the fundamental notes themselves which differed by one quarter vibration only.

The 64,000 estimate given by Perrett may therefore easily be two, three or four times too great. Further it should be noted that fine differentiation can only occur over a relatively small range, for, in the extreme bass and to a greater extent in the extreme treble, differentiation is much impaired. Relatively few fibres would suffice for audible acuity near the top and bottom limits and this fact greatly reduces the calculated total number that should be present. McKendrick, taking this point into consideration, estimated the total number of different pitches to be roughly 11,000, *i.e.* about the same number as there are vibrating elements of the cochlea. Even if, however, it was found that the number of vibrating elements in a human ear was definitely less than the number of different pitches that the ear of another individual could perceive, that would not constitute evidence against the resonance theory; to obtain evidence it would be necessary to count the number of pitches and the number of fibres for one and the same ear of the same individual and to find the former number so much larger than the latter as to be outside the range of experimental error.

4.

Perrett has inferred (*loc. cit.* p. 36) that the Helmholtz hypothesis is unable to explain how the ear can appreciate the *staccato* ending of a note; he has further stated (p. 38) that it is out of all reason to suppose that if there are resonators in the cochlea a loud note can be reduced to inaudibility in the same time as a soft note. My experiments with the model shown in Plate II indicate that so long as the to and fro motion of the levers continues, so long does the 'in tune' resonator retain the maximum amplitude of its swing. But if the motion be discontinued, then this resonator gradually comes to rest, the other resonators having remained at rest all the time. Now Helmholtz concluded from his experiments on trills and beats that in the case of the resonators in the cochlea the intensity of vibration would be reduced to one-tenth its original value after 9·5 unaided vibrations had occurred following the cessation of the musical note originating them. In the case of the mechanical model shown in the figures, a very much greater number had to occur before the amplitude of the swing was reduced to one-tenth. This in no way invalidates Helmholtz's value, because the resonators of the model, since they swing in air, are very lightly damped; whereas the damping of the resonators of the cochlea is much greater because they are immersed in liquid. When, therefore, a musical note ceases, the amplitude of the 'in tune' vibrator diminishes at a rate which is largely dictated by the degree of damping, and the silence cannot be perceived until the amplitude has so far diminished that, *contrasted* with the amplitude of the 'in tune' vibrators during the sounding of the note, the stimulus to the hair cells has become negligible. In other words, apart altogether from the phenomena (largely physical in their characters) which control the amplitude of vibration of any particular resonator in the cochlea, there are in addition the general physiological characters of the sense organs to be taken into account; since there is no reason to suppose that the hair cells are governed by conditions differing in any way from those which operate in the case of sight or touch. It would be expected therefore that simultaneous and successive contrast, phenomena so marked in their effects in the other sense organs, would operate also in the case of hearing. Thus, if in the case of the eye the illumination of a white surface at which the eye is looking is suddenly reduced to one-hundredth part at first the illumination appears to have fallen to zero, but as the eye adapts itself to the new conditions, the visibility of the white surface rapidly improves again. So, in the case of the ear, if, after a period of twenty vibration intervals from the cessation of a very loud sound, the resonators of the ear are still actually in oscillation, it may easily be that for the hair cells the sensation of motion will virtually have ceased, because the amplitudes of the oscillations have fallen to so small a value compared to those that were observed a few moments before. The statement made by Perrett[1] that the vocal harmonies are audible after the voice has stopped singing, *i.e.* while certain hair cells are suffering from the effects of successive contrast, is readily explained on the ground that resonators and hair cells involved in the perception of the vocal harmonies are different from those affected by the previous sound waves. Further there seems to be no reason why a loud note cannot be reduced to inaudibility in the same time as a soft note, for Helmholtz showed that in ten vibrations the amplitude is reduced to less than one-tenth. The rate of reduction is therefore unaffected by the initial amplitude of the vibrations. Whatever value this may have at first, the time taken for it to fall to a certain fraction will always be the same, in other words the fall is a geometric one. The same would appear to be true in the case of a sense organ affected by successive contrast, namely, that whatever value the initial stimulus may have a reduction to a certain fraction of that stimulus will cause it to fall below the (temporary) threshold value of the sense organ. Combining this with the preceding statement, we may say that the time taken for a stimulus falling geometrically to reach the threshold of a sensory end organ influenced by successive contrast is approximately the same for all stimuli whether they be large or small. That is to say, a loud sound and a soft sound will cease to be heard at the same instant.

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[1] Perrett, *Nature*, Nov. 7, 1918, 185.

5.

Perrett asks (*loc. cit.* p. 25),—Since the more accurately each resonator of the cochlea is tuned the greater will be the number of periodic impulses required to produce the full effect, how then does the resonance theory explain the fact that we can hear a note beginning at full strength and then gradually diminishing in intensity, such as a piano note which is powerful only at the moment when it is struck and rapidly decreases in strength?

It has been shown above (see Plate II, fig. 1) that the first complete vibration (corresponding in its period to fifty-five per minute) is sufficient to bring the top vibrator of eighty per minute completely and the bottom vibrator of thirty-eight nearly to rest. After four vibrations all except the three vibrators above and below the chosen one are at rest as the figure shows. After ten vibrations one only on either side is left in vibration. Even with this mechanical model in which the impulse is conveyed by shifting the point of support first to one side and then to the other of the centre of gravity, the rate at which the 'out of step' vibrators are eliminated is very rapid. If the vibrators could be immersed in liquid as they are in the ear and if the motion could be conveyed to the liquid itself, a much more rapid elimination would occur, because as soon as one of the 'out of step' vibrators lagged behind or got ahead of the oscillation of the liquid, the eddies set up would immediately bring it to rest. Calculation shows that in a beneficial case eight complete vibrations would limit the 'out of step' vibrators to those of one semi-tone on either side of the chosen one (eight complete vibrations would occur in ·03 sec. approximately in the case of the middle *C* of the musical scale). After this short interval of time, therefore, not only is the total number of vibrating resonators limited to one semi-tone on either side of the 'in tune' one, but also, as may be shown by calculation, the latter has attained nearly 90 per cent. of its final amplitude. Any changes in the loudness of the musical note occurring after this interval of ·03 sec. will be faithfully followed (with of course a small time lag). The resonance theory can thus explain how it is that we hear a note beginning at full strength and diminishing in intensity, as easily as we can hear a note beginning soft and becoming louder or another keeping at the same intensity.

6.

Lastly Perrett (*loc. cit.* p. 25) has objected that, since a longer time must be required for the vibrators of the cochlea to reach their maximum amplitude in the case of a low note than in that of a high, "the perception of a bass note on the piano would therefore lag behind that of a note in the treble, if the two were struck simultaneously." It should be noticed, however, that the perception of a sound begins not from the time that its 'in tune' resonator reaches its maximum amplitude, or even from the instant when the eighth vibration has been completed, but from the beginning of the first vibration of that resonator. It may be true that this beginning will consist partly of the noise created by the accompanying vibrations of certain other 'out of tune' resonators, but almost before the hair cells have accommodated themselves to the onset of the note from silence these 'out of tune' resonators have ceased to vibrate and have left the pure note to continue.

7. SUMMARY.

(1) It has been stated that the Helmholtz hypothesis of audition is unable to account for certain properties of the organ of hearing: first, its ability to appreciate a short interval of silence and to differentiate 'utter' from 'udder.'

I find by experiments with a syren and a resonator apparatus that the ear perceives a short silence as a short noise and that this perception is readily explained on the Helmholtz basis.

(2) Objections have been made that the difference in the widths of the different parts of the basilar membrane is quite insufficient to permit the vibration of one part in response to high notes and of another part in response to low. I find that besides width, mass and tension will affect the period of vibration of the basilar membrane. If these are also taken into account the perception of high and low notes is readily explicable.

(3) The statement that the number of separate elements in the basilar membrane is too few to account for the number of different pitches which the ear perceives appears to be without foundation.

(4) The objection that on the Helmholtz basis the ear should not be able to perceive a note ending *staccato* can be met if the effects of successive contrast are taken into account.

(5) The resonator theory can explain the perception of a note beginning at full strength.

(6) The resonator theory can also explain how it is that treble and bass notes sounded and terminated together are heard to begin and to end simultaneously.

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A MINOR STUDY OF NYCTOPSIS.

By J. C. FLÜGEL.

(*From the Psychological Laboratory, University College, London.*)

I. Introduction.

Although the measurement of an individual's ability to see in daylight has for some considerable time constituted an essential part of most medical examinations, the corresponding measurement of Nyctopsis, or Night Vision, has usually received but scant attention. During the War, however, it became manifest that it was a matter of considerable military importance to appoint to certain tasks only such persons as possessed good powers of seeing in the dark and to exclude from these tasks all who suffered from nyctalopia or whose nyctoptic ability fell below a certain standard. It therefore became necessary to devise some test by which individuals with the required nyctoptic qualifications could be conveniently selected. With this end in view, a study of Nyctopsis was included in the various branches of war research undertaken in the Psychological Laboratory of University College, London, under the direction of Professor Spearman; the results here recorded being a portion of the data obtained from this study.

In this paper we shall deal only with the *relative* abilities of different individuals and with such conclusions as can be drawn from a consideration of their relative abilities. The accurate determination of an individual's absolute ability, in terms of stimulus intensity, and of the shape of his 'adaptation' curve—depending as these do upon delicate photometric measurements—is a matter which demands very considerable care and precision and which could not be undertaken with the desirable degree of thoroughness at a time when all the energies of the very small laboratory staff were required for the solution of urgent practical problems in a variety of fields. The consideration of these matters will therefore be left to a second paper[1] by my colleague Dr Ll. Wynn Jones, who continued under more convenient circumstances the work begun by me and whose method permitted of greater accuracy and refinement than mine. Measurements of relative nyctoptic ability are, however, comparatively easy and the results obtained in these earlier experiments of mine are, it would appear,—in spite of the necessarily somewhat rough and hurried manner in which these experiments were carried out—not altogether wanting in accuracy or in interest. In view of the comparatively little study that has hitherto been devoted to the subject, the publication—in brief form—of these results, as a preliminary to the report of Dr Wynn Jones's investigations, seems therefore to be justified.

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[1] This *Journal*, 1921, XI, 299–319.

Before passing on to a description of the experiments, I take the opportunity of acknowledging my indebtedness to Prof. Spearman, without whose very valuable guidance and assistance the research could not have been made, and to Dr Wynn Jones who, as already indicated, carried on the investigation when—owing to other claims upon my time—I was no longer able to do so, and who freely gave me the benefit of his advice as to the form in which the results could most conveniently be dealt with and expressed.

II. METHODS.

Four distinct experiments were undertaken in the work here reported.

(1) Visual acuity in ordinary daylight, as measured in the usual way by the ability to read Snellen's types at a distance of six metres, the individual ratings being made on the basis of the number of letters correctly read. For convenience of description this test will be referred to as θ[1].

(2) Light sensitivity, *i.e.* ability to distinguish light from darkness. A 'pointolite' electric lamp of 100 candle power was enclosed in a box with a single aperture which permitted the rays from the lamp to illumine a square patch of the (black) wall of the dark room, the patch being of such a size that it subtended an angle of 30° from the eye of the observer. The intensity of the illumination could be varied within very wide limits by means of a suitable resistance, and the aim of the experiment was to determine the lowest illumination at which the observer could just distinguish whether the patch was illuminated or not. For this purpose the rays of light were intercepted at irregular intervals by a black cardboard screen being placed noiselessly in front

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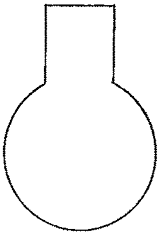
[1] The abbreviation employed to designate this and the other tests agrees with that used for the same purpose for the most nearly corresponding tests of Dr Wynn Jones; but when comparing the present results with those obtained by him, it must throughout be borne in mind that the conditions under which the two sets of results were obtained only roughly correspond, there being in every case important differences between his experiments and mine.

of the aperture of the box (and in such a position as to be itself invisible to the observer); the observer receiving the instruction to say "Dark" whenever the light was intercepted and "Light" whenever the intercepting screen was removed. For convenience of reference this experiment will be designated *P*.

(3) Visual acuity in dim light, as measured by the ability to perceive the position of certain figures. The figures chosen for this purpose were roughly of the shape of a square resting on a circle (as indicated in the accompanying diagram), the side of the square being of the same length as the radius of the circle. Twelve such figures of varying size were constructed, the smallest having a radius of ·66 mm., the largest one of 76 mm.; each figure being of such a size that its radius was longer by approximately 50 per cent. than that of the next smaller figure. The figures were cut out of stiff white cardboard and were fixed (in three columns and in order of their magnitude) to a large sheet of dull black cardboard in such a way that they could be freely rotated about the centre of their circular portions but would yet remain in whatever position they were placed. The task of the observers was to detect the position of the projecting square portions of the figures. The observers' judgments were communicated to the experimenter by treating the projections as if they were the hour hands of clocks; thus an upward vertical position of the projection was designated '12 o'clock,' a downward vertical position '6 o'clock,' a rightward horizontal position '3 o'clock,' etc. The whole series of figures on their cardboard mount was evenly illuminated by the 'pointolite' lamp, which (in contra-distinction to experiment *P*) was made to give a light of constant intensity throughout the experiment, this intensity being in the neighbourhood of ·1 candle power. This experiment will be referred to as *X*.



(4) This experiment (which will be designated *Y*) was in all respects similar to *X*, except that the illumination was increased by about 100 per cent., corresponding thus roughly to ·2 candle power.

The actual procedure adopted in the experiments was as follows:

The observer, having arrived in the laboratory in a light-adapted condition (all the experiments were conducted in the morning or early afternoon), was immediately put through the Snellen daylight test. He then entered the dark room and for a period of 2 minutes gazed continuously at a large white sheet illuminated by the rays of an electric lamp of 1000 candle power; the object of this being to ensure approximate uniformity of light-adaptation before beginning the process of dark-adaptation. The observer then took his seat at a window communicating with a second dark room, in which was placed the apparatus already described and in which the experimenter was stationed. This window was normally covered by a close-fitting dark curtain, so that no light penetrated to the eye of the observer, even when the experimenter's room was brightly illuminated.

The 1000 candle power lamp being extinguished, the observer waited in the darkness during a period of 1 minute, after which, on receiving a signal from the experimenter, he lifted the curtain and gazed upon the illuminated patch on the dark room wall in preparation for experiment *P*. This illuminated portion of the wall was situated directly opposite the observer's window and at a distance of 2 m. from his head, he being provided with a chin rest in order that this distance might remain constant throughout the experiments. The experimenter now proceeded to intercept the light in the manner described. After four movements of the intercepting screen (*i.e.* two of interception and two of withdrawal), he reduced the intensity of the illumination by a predetermined amount (this being facilitated by means of small strips of luminous paper pasted on to the resistance apparatus at certain points) and repeated the four movements of the screen. This procedure was continued until the observer had failed to report (or had wrongly reported) six movements in succession.

The experimenter then instructed the observer to close the curtain. As soon as this was done, he turned on the ordinary lighting in his own room, noted the threshold of the observer (in terms of resistance), recorded the total number of correct judgments and quickly made the necessary preparations for experiment *X*, by placing the cardboard sheet with the white figures in the path of the rays from the 'pointolite' lamp and by adjusting the intensity of the light from this lamp to the (higher) degree required for the new experiment. He then extinguished the ordinary room lights and gave the signal to the observer for the curtain to be raised; whereupon experiment *X* began. During this the experimenter pointed with a small wand of luminous paper to each of the figures in turn (from large to small progressively), the observer indicating in the way already mentioned what he judged to be the position of the figures. The experimenter counted the number of correct judgments and, at the close of the series, interposed his body between the observer's window and the figures and quickly removed these latter into fresh positions; after which the process of judging immediately began again. Two such repetitions were undertaken, making three series of judgments in all.

The total number of correct judgments in the three series afforded the measure of the observer's ability[1].

The curtain was then closed once more, whereupon the experimenter adjusted the lighting to the (still higher) intensity required for experiment *Y*. Experiment *Y* was then conducted in precisely the same manner as experiment *X*.

With a little practice on the part of the experimenter, it was found possible to conduct the whole set of experiments *P*, *X* and *Y*, as just described, in a period not exceeding 2½ minutes in duration. This set of experiments being completed, the observer rested until there had elapsed a period of 10 minutes from the time when he had been first exposed to the darkness. The whole set of experiments *P*, *X* and *Y* was then repeated and similar repetitions were undertaken after the lapse of 20, 30, 40 and 50 minutes. Six measurements were thus obtained for each kind of nyctoptic ability, so that the observers could be compared with one another at all stages of adaptation. The whole test for each individual (including the daylight experiment (θ), and the preliminary adaptation with the 1000 candle power lamp) occupied thus about one hour.

All the experiments (including θ) were performed binocularly.

Twenty-three observers took part in the experiments, of whom five were personal friends of the experimenter, fourteen students of University College and four boys from Stanhope Street (London County Council) School—eleven of these being males and twelve females, the ages varying from about 12 to over 70 years. I am much indebted to all the observers for their kind and patient collaboration.

III. RESULTS.

The results of the experiments were in the first place recorded in a table representing the number of correct judgments made by each observer in every test. This table need not be reproduced here, as the results would have to be translated into luminosity values before they would become of general interest in themselves, and this process of translation would, for the reasons already stated, be of somewhat doubtful accuracy in the present case. The conclusions that concern us here can be most conveniently expressed in the form of correlation coefficients. Such coefficients (calculated by the product-moments formula) were therefore worked out for all possible correlations between the different tests and are presented in the accompanying table, in which θ, P, X and Y indicate the experiments above described, while the numbers after these letters refer to the time that had elapsed between the beginning of dark-adaptation and the beginning of the particular test concerned.

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[1] To save time, whenever it appeared that the largest figures could be judged without difficulty, no judgments were made for these figures, the score being calculated as if correct judgments for these figures had actually been recorded; similarly, whenever three incorrect judgments in succession had been recorded, no further judgments were made in that series, the score being counted as if no judgments (or incorrect judgments) had been made for the remainder of the figures.

The principal conclusions that may be drawn from a study of this table are as follows:

(1) A satisfactory guarantee of the reliability of the experiments for our present purpose is afforded by the very high correlations between the measurements for 40 mins. and 50 mins. in the case of P, X and Y. Thus $\frac{P\,40}{P\,50}$ (as the correlation between $P\,40$ and $P\,50$ may conveniently be written) $= \cdot 95$, $\frac{X\,40}{X\,50} = \cdot 87$ and $\frac{Y\,40}{Y\,50} = \cdot 94$.

These high correlations show at the same time that for practical purposes in examining individuals from the point of view of nyctoptic ability, it is probably unnecessary to prolong the examination beyond 40 minutes.

(2) This last consideration leads naturally to the question as to whether a shorter period than 40 minutes will suffice for such examination. Our present data are of course insufficient for a final answer to this question, but some interesting results bearing on the problem are afforded by an inspection of the inter-correlations between the different measurements of P, X and Y respectively (shown in the three sections of the table running diagonally from top-left to bottom-right). The general tendency to positive sign and the frequency of high values show at once that the order of the twenty-three observers tends to be the same in the successive tests and that some information as to the true position of any individual is therefore provided by nearly every one of the tests. If, with a view to greater precision, we take the measurements after 50 minutes as being those most nearly representing nyctoptic ability after complete dark-adaptation (and as being therefore the most important—both theoretically and practically—for the majority of purposes) and compare with these the measurements after 40, 30, 20, 10 minutes and 1 minute respectively, we note that there is a tendency for the correlations to decrease steadily in magnitude (see sixth or bottom row of each of the above-mentioned sections of the table). The same statement holds good, if, instead of using the 50 minute measurements as our

Table of Correlations between (1) *visual acuity in daylight* (θ); (2) *light sensitivity* (*P*); (3) *visual acuity in a dim light* (*X*); (4) *visual acuity in a slightly brighter light* (*Y*); *after varying periods of dark-adaptation* (*figures represent adaptation times in minutes*). (*Probable errors are indicated in brackets.*)

	θ	P 1	P 10	P 20	P 30	P 40	P 50	X 1	X 10	X 20	X 30	X 40	X 50	Y 1	Y 10	Y 20	Y 30	Y 40	Y 50
θ		—·17 (·137)	—·06 (·140)	·08 (·140)	·06 (·140)	·08 (·140)	·02 (·141)	·47 (·11)	·73 (·065)	·83 (·044)	·71 (·07)	·76 (·06)	·79 (·054)	·66 (·08)	·62 (·085)	·64 (·082)	·60 (·09)	·63 (·085)	·56 (·097)
P 1	—·17 (·137)		·43 (·115)	·02 (·141)	—·35 (·124)	—·32 (·124)	—·26 (·13)	·19 (·137)	·02 (·141)	·12 (·138)	·08 (·140)	—·06 (·140)	·08 (·140)	·10 (·139)	·11 (·139)	·26 (·13)	·10 (·139)	·05 (·140)	·02 (·141)
P 10	—·06 (·140)	·43 (·115)		·14 (·137)	·49 (·112)	·31 (·124)	·44 (·115)	·27 (·13)	·27 (·13)	·22 (·135)	·19 (·137)	·08 (·140)	·04 (·140)	·26 (·13)	·21 (·135)	·35 (·124)	·34 (·124)	·36 (·124)	·37 (·123)
P 20	—·08 (·140)	·02 (·141)	·14 (·137)		·79 (·054)	·69 (·074)	·65 (·083)	·35 (·124)	·10 (·138)	·36 (·124)	·07 (·140)	·16 (·137)	·07 (·140)	·24 (·135)	·26 (·13)	·26 (·13)	·29 (·129)	·24 (·135)	·34 (·124)
P 30	·06 (·140)	—·35 (·124)	·49 (·112)	·79 (·054)		·85 (·04)	·77 (·06)	·28 (·129)	·38 (·123)	·26 (·13)	—·10 (·139)	·11 (·139)	·02 (·141)	·38 (·123)	·41 (·118)	·35 (·124)	·36 (·124)	·35 (·124)	·36 (·124)
P 40	·08 (·140)	—·32 (·124)	·31 (·124)	·69 (·074)	·85 (·04)		·95 (·014)	·13 (·138)	·09 (·138)	·01 (·141)	—·07 (·140)	·19 (·137)	·09 (·140)	·23 (·135)	·35 (·124)	·25 (·135)	·39 (·123)	·32 (·124)	·36 (·124)
P 50	·02 (·141)	—·26 (·13)	·44 (·115)	·65 (·083)	·77 (·06)	·95 (·014)		·10 (·138)	·08 (·140)	·02 (·141)	—·06 (·140)	·22 (·135)	·11 (·139)	·16 (·137)	·29 (·129)	·34 (·124)	·37 (·123)	·33 (·124)	·45 (·115)
X 1	·47 (·11)	·19 (·137)	·27 (·13)	·35 (·124)	·28 (·129)	·13 (·138)	·10 (·138)		·27 (·137)	·11 (·139)	·27 (·13)	·26 (·13)	·33 (·124)	·55 (·099)	·52 (·105)	·43 (·114)	·26 (·13)	·20 (·135)	·35 (·124)
X 10	·73 (·065)	·02 (·141)	·27 (·13)	·10 (·138)	·38 (·123)	·09 (·138)	·08 (·140)	·27 (·13)		·77 (·06)	·35 (·124)	·61 (·087)	·69 (·074)	·65 (·083)	·62 (·087)	·63 (·085)	·68 (·076)	·75 (·06)	·74 (·065)
X 20	·83 (·044)	·12 (·138)	·22 (·135)	·36 (·124)	·26 (·13)	·01 (·141)	·02 (·141)	·11 (·139)	·77 (·06)		·74 (·065)	·76 (·06)	·72 (·07)	·61 (·087)	·55 (·099)	·59 (·091)	·79 (·054)	·65 (·083)	·61 (·087)
X 30	·71 (·07)	·08 (·140)	·19 (·137)	·07 (·140)	—·10 (·139)	—·07 (·140)	—·06 (·140)	·27 (·13)	·35 (·124)	·74 (·065)		·85 (·038)	·80 (·05)	·40 (·118)	·48 (·112)	·50 (·105)	·47 (·11)	·46 (·11)	·44 (·118)
X 40	·76 (·06)	—·06 (·140)	·08 (·140)	·16 (·137)	·11 (·139)	·19 (·137)	·22 (·135)	·26 (·13)	·61 (·087)	·76 (·06)	·85 (·038)		·87 (·037)	·46 (·11)	·53 (·105)	·55 (·099)	·59 (·091)	·54 (·099)	·50 (·105)
X 50	·79 (·054)	·08 (·140)	·04 (·140)	·07 (·140)	·02 (·141)	·09 (·140)	·11 (·139)	·33 (·124)	·69 (·074)	·72 (·07)	·80 (·05)	·87 (·037)		·53 (·105)	·61 (·087)	·61 (·087)	·67 (·076)	·63 (·085)	·60 (·087)
Y 1	·66 (·08)	·10 (·139)	·26 (·13)	·24 (·135)	·38 (·123)	·23 (·135)	·16 (·137)	·55 (·099)	·65 (·083)	·61 (·087)	·40 (·118)	·46 (·11)	·53 (·105)		·88 (·037)	·77 (·06)	·80 (·05)	·74 (·065)	·77 (·06)
Y 10	·62 (·085)	·11 (·139)	·21 (·135)	·26 (·13)	·41 (·118)	·35 (·124)	·29 (·129)	·52 (·105)	·62 (·087)	·55 (·099)	·48 (·112)	·53 (·105)	·61 (·087)	·88 (·037)		·85 (·038)	·85 (·038)	·82 (·045)	·86 (·036)
Y 20	·64 (·082)	·26 (·13)	·35 (·124)	·26 (·13)	·35 (·124)	·25 (·135)	·34 (·124)	·43 (·114)	·63 (·085)	·59 (·091)	·50 (·105)	·55 (·099)	·61 (·087)	·77 (·06)	·85 (·038)		·90 (·027)	·93 (·019)	·89 (·037)
Y 30	·60 (·09)	·10 (·139)	·34 (·124)	·29 (·129)	·36 (·124)	·39 (·123)	·37 (·12)	·26 (·13)	·68 (·076)	·79 (·054)	·47 (·11)	·59 (·091)	·67 (·076)	·80 (·05)	·85 (·038)	·90 (·027)		·96 (·012)	·96 (·012)
Y 40	·63 (·085)	·05 (·140)	·36 (·124)	·24 (·135)	·35 (·124)	·32 (·124)	·33 (·124)	·20 (·135)	·75 (·06)	·65 (·083)	·46 (·11)	·54 (·099)	·63 (·085)	·74 (·065)	·82 (·045)	·93 (·019)	·96 (·012)		·94 (·017)
Y 50	·56 (·097)	·02 (·141)	·37 (·123)	·34 (·124)	·36 (·124)	·36 (·124)	·45 (·115)	·35 (·124)	·74 (·065)	·61 (·087)	·44 (·118)	·50 (·105)	·60 (·087)	·77 (·06)	·86 (·036)	·89 (·037)	·96 (·012)	·94 (·017)	

standard, we use the 40 minute or 30 minute measurements for this purpose (fifth or fourth rows respectively). This indicates that the shorter we make the experiment, the less accurate our results will be—as measurements of the observers' ultimate nyctoptic power. Just how much the experiments can be shortened depends of course upon the degree of accuracy required. From our present results (which of course are insufficient for the establishment of any definite rule) it would appear that in the case of *P* fairly accurate measurements can be taken after 20 minutes and in the case of *X* after 10 minutes, while in the case of *Y* even the first measurement—that taken after 1 minute—correlates highly with the ultimate record after 50 minutes.

An inspection of the original table of the records of the observers' performances (not given here) reveals the fact that the relatively low (or negative) correlations of *P* 1 and *P* 10 with *P* 30, *P* 40 and *P* 50 are largely due to the results obtained, (*a*) from one observer who suffered from unusually slow power of adaptation and who therefore did badly in the earlier tests, but who ultimately took a high place as regards nyctoptic ability, and, (*b*) from three observers who, though good at initial adaptation, made very slow progress in the later tests. If these results can be regarded as at all typical, they would seem to indicate that there are two small classes of observers whose anomalous behaviour reduces the correlation between early and later measures of light sensitivity, *i.e.* those whose curves of adaptation show an unusually slow or an unusually rapid rise at the beginning, as compared with their ultimate performance[1].

(3) Visual acuity in daylight (θ) does not correlate significantly with light sensitivity (*P*) at any adaptation time. Practically, this result is of considerable importance, as it shows that no inference whatever can be drawn from a person's performance in the usual Snellen test as to his ability to see in a very dim light. Theoretically, it is of interest as affording evidence for the independence of the mechanisms involved in the two kinds of visual ability—an independence such as is demanded by the Duplicity Theory of Vision.

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[1] It is noteworthy that three of the four observers here referred to have the three lowest of all our twenty-three records for daylight vision (θ). If there is any significance in this, it is probable that experimentation with a selected group of persons who had previously passed a fairly rigorous test for daylight vision would reveal a higher correlation between the earlier and later results than was found in the present investigation. The results obtained by Dr Wynn Jones lend some support to this view.

A correlation between faulty visual acuity in daylight and anomalous dark-adaptation would of course be of considerable interest on its own account, if proved.

(4) Visual acuity in daylight (θ) correlates moderately with visual acuity in a dim light (both X and Y). Practically, this means that those who do well in the Snellen test tend to have good visual powers also in a rather feeble light, though no safe inference can be made in any individual case. Theoretically, this result is of course perfectly reconcilable with the Duplicity Theory, if we suppose—as we should probably do in any case—that both rods and cones are to some extent active under illumination of the intensity employed in experiments X and Y, and that the mechanism of refraction which plays such an important part in θ is also to some extent operative in X and Y. The fact that θ tends to correlate somewhat more highly with X than with Y would be difficult to account for, if corroborated, but the significance of this difference is doubtful in the present case, as it is only about twice the size of its probable error.

(5) Light sensitivity (P) tends to correlate slightly with visual acuity in dim light (X and Y). From the practical point of view this correlation (if genuine) is quite insufficient as a ground for drawing any helpful inference from an individual's performance in X or Y as to his ability in P or *vice versa*. Theoretically, this result may perhaps be regarded as complementary to that recorded under (4). Just as there it appeared that the mechanisms involved in daylight vision were probably to some extent involved in vision in a rather dim light, so here it seems that the latter vision involves also to some extent the mechanisms that come into play in dealing with visual stimuli of quite minimal intensity; though it should be noted that the participation of these latter mechanisms in dim vision is (if we may judge from the magnitude of the correlations) less than that of the daylight mechanisms $\left(i.e.\ \frac{P}{X} < \frac{\theta}{X}\right.$ and $\left.\frac{P}{Y} < \frac{\theta}{Y}\right)$.

(6) Visual acuity in dim light (X) correlates only moderately with visual acuity in dim light of rather higher intensity (Y).

This result (which is not due merely to the unreliability of the measures correlated, as is shown by the—quite significantly—higher values of the inter-correlations within the X and Y groups themselves), if corroborated, is of some practical importance, since it indicates the necessity for caution in inferring from a person's ability in one degree of dim light his ability in illumination of rather higher or lower intensity.

Theoretically, we should be inclined at first sight to account for the relatively low correlations between X and Y, as compared with the higher ones within the X and Y groups themselves, on the assumption that X and Y differed as regards the relative proportions in which the mechanisms of daylight vision (corresponding to θ) and the mechanisms of true night vision (corresponding to P) were involved. Thus it would be easy to suppose that X involved the latter (P) mechanisms to a greater extent and the former (θ) mechanisms to a less extent than did Y. As against this interpretation, however, it must be pointed out that the correlations between P and Y are higher than those between P and X and that (as we have already observed) the correlations between θ and X are higher than those between θ and Y—in both cases contrary to what on *a priori* grounds might have been expected (though in both cases the differences in question are only of about twice the magnitude of their probable errors). Possibly further investigation will demonstrate (as has so often been the case before) that the real state of affairs is more complex than had been contemplated by current theories and that instead of one there are two or more distinct nyctoptic mechanisms which come into play at different degrees of stimulation.

The present data are however quite insufficient to serve as the basis of any elaborate theoretical constructions. All I can do here is to point out that the method here adopted (*i.e.* the use of correlation with data obtained by a relatively rough and simple procedure) is capable of throwing very considerable light upon questions of both practical and theoretical importance within the sphere of psycho-physics with which I have been dealing.

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A METHOD OF MEASURING NYCTOPSIS WITH SOME RESULTS.

By LL. WYNN JONES.

(From the Psychological Laboratory, University College, London.)

I. *Introduction* (pp. 299–301).
II. *Apparatus for testing nyctopsis* (pp. 301–304).
III. *Method of procedure* (pp. 305–308).
IV. *Results* (pp. 308–316).
V. *Summary of results* (p. 316).
VI. *Tables* (pp. 317–319).

I. INTRODUCTION.

A TEST for Nyctopsis or Night-vision has not hitherto been included among the visual tests which are usually employed in vocational selection. It is true that taxi-cab drivers have to pass a sight test on first obtaining their licences, but there is no periodic re-examination, and there is no test for night-vision or night-blindness imposed. It is notorious also that anyone may obtain a licence to drive a private or trade motor-car, not only without passing a night-blindness test but without passing a vision test of any kind. On further ascending the velocity scale we come to the driver of a night express. In his case, again, so far as I am aware, there is no test for night-blindness. Finally we come to the trained aviator who has to approach an object at twice the speed of an express train. His work is of so exacting a nature that it requires a high standard of physical fitness. Especially so with respect to his vision. If he flies at night, a test for nyctopsis becomes not merely desirable but essential.

The primary object of the following experiments was to gather data towards the establishment of an administrative test for nyctopsis for night pilots in the Royal Air Force. It was found that whereas one pilot may experience difficulties in flying by night, another may have a brilliant record for night-flying, although both may be equally endowed as regards their visual acuity in daylight, as shown by the results of the standard eye tests which they have passed.

It would, therefore, be very desirable if a test were devised which would detect not only those persons who suffered from night-blindness, but also those whose nyctoptic capacities fell below certain standards. Before such a test can be established it is evident that a considerable amount of research must be undertaken in order to fix on the standards to be demanded. Quite apart, however, from the military requirements which gave rise to the research, the above considerations show that nyctopsis is a field of study of wide application in view of the increase both in speed and in volume of land, sea and air traffic. It may well be that a study of the nyctopsis of miners will prove of theoretical and practical value.

Twelve observers took part in the experiments[1]. All were officers in the Royal Air Force with experience as pilots overseas. To them and to those of my friends who acted as observers in the preliminary experiments, I am much indebted for their patient assistance. I wish also to express my thanks to my Commanding Officer, Wing Commander Martin Flack, C.B.E., Royal Air Medical Service, for his assistance in many ways and for his interest in the progress of the work.

Before describing the apparatus I may be allowed to discuss briefly what is generally known as night-blindness. In its widest sense the term denotes that condition in which one sees well by day, but by night or under feeble illumination sees poorly or not at all. We may distinguish between three main forms of night-blindness.

1. *Night-blindness due to diseases producing opacities in the media.* Opacities in the media excite the symptoms of night-blindness when they occupy the periphery and leave the centre free, as in the case of peripheral opacities of the cornea and lens. Under brilliant illumination, when the pupil is contracted, these opacities no longer fall within the area of the pupil, while when the illumination is diminished and the pupil is dilated, they fall within the area of the pupil and interfere with sight. Again, when there are faint diffuse opacities distributed uniformly over the entire cornea, the sight is often better when the pupil is contracted because the dazzling due to diffused light is then less. In all these cases, therefore, the width of the pupil is what determines

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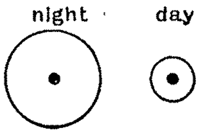
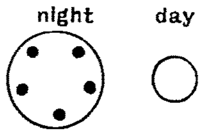
[1] The experiments here recorded were carried out at the Psychological Laboratory of University College, London, under the direction of Professor Spearman, to whom my warmest thanks are due, both for devising the necessary apparatus and for his invaluable advice and guidance throughout the course of the work. I wish also to express my indebtedness to my colleague Mr Flügel who had already been engaged on the same problem and who very kindly gave me the benefit of his experience and allowed me the perusal of his protocols.

the visual power. The following diagram will, perhaps, make this clearer.



Central opacity with day-blindness.



Peripheral opacities with night-blindness.

Note. The black dots represent opacities, and the circles the pupillary area.

2. On the other hand, *night-blindness due to disease of the light-perceiving apparatus* has nothing to do with the width of the pupil. The diseases to be considered in this connexion are idiopathic night-blindness on the one hand, and on the other hand retinitis pigmentosa, together with other causes of atrophy of the retina, *e.g.* such as occurs after retinitis, choroiditis, glaucoma. The nature of the visual disturbance, however, in these categories is essentially different. In idiopathic night-blindness there is a reduction of the light sense, probably as a result of insufficient regeneration of the visual substances: there is a disturbance of nutrition of the retina. Now the fovea centralis has a feebler light sense than the periphery of the retina, and hence in idiopathic night-blindness stops functioning simultaneously with the periphery or even somewhat earlier if the illumination is reduced. Many patients state that when the illumination is feeble they see a dark spot in the centre of the visual field. In retinitis pigmentosa the reverse obtains. Here the periphery of the retina is diseased and, when the illumination is reduced, it no longer functions, while central vision is still intact. Then the visual field becomes so small that it no longer suffices for the patient's orientation[1]. Under this head we may conveniently consider hereditary or congenital night-blindness which appears to be due to a defective power of the rods to respond to the stimulus of a feeble light.

3. *Traumatic night-blindness*. The concussion following modern explosives, for example, leads to a great variety of fundus lesions. The injuries are caused by the violence communicated through the bones of the skull or by air vibration.

II. Apparatus for testing Nyctopsis.

When the research was started we had the following aim in view—to devise an exact apparatus for the purpose of investigation in general, and for ascertaining the minimum requirements of any administrative test. The following problems amongst others immediately presented themselves—the relation of daylight visual acuity to the acuity of the dark-adapted eye: a comparison of central and peripheral vision for the dark-adapted eye: the accurate determination of an individual's sensitivity to light in terms of the intensity of the stimulus for various degrees of adaptation. In addition it was hoped to study various individual differences in some or all of these problems. The apparatus eventually employed we shall now proceed to describe.

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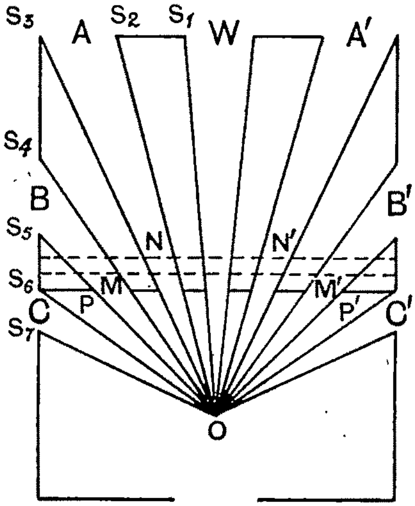
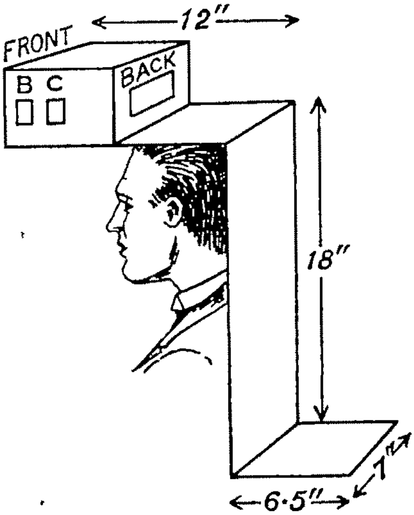
[1] Fuchs's *Textbook of Ophthalmology*, transl. by Duane, 1919, p. 635.

(1) *The Nyctoptometer*. This is a rectangular box made of 3-ply wood measuring 6 inches long by $4\frac{5}{8}$ inches broad by $3\frac{3}{16}$ inches high. Inside is a Siemens 20 candle-power, gas filled lamp (11·3 volts, 33 mm. bulb):



It is absolutely necessary in these experiments to have *standard lamps* which give constant illumination as long as the current passing through is constant. Theoretically any lamp which gives the required luminosities would answer the purpose as long as a graph connecting its luminosity with the current passing through was provided. But in practice it is a great advantage to have lamps so accurately standardised that each gives the same illumination for any given current. Through the courtesy of Messrs Siemens we were allowed to choose lamps which gave very nearly the same candle-powers for a given current. This was done in our presence at the Research Laboratory of Messrs Siemens, and I am indebted to Mr Le Maréchal for his kindness in superintending the test. I may add that the lamps were already standardised to a certain degree of accuracy, though not to that extreme degree which was desirable for our purpose; hence the necessity for the test just mentioned. One lamp has been kept as a standard at the Psychological Laboratory of University College. The lamp used in the following experiments was compared with this standard and found to be in the same condition after the experiments as it was at the beginning.

In the diagram (p. 302) which is a horizontal section through the middle of the box W, A, A', B, B', C, C' are windows each subtending an angle of 10° at the point O where the tungsten filament of the lamp cuts the horizontal section. As the filament can be regarded as a short vertical line, the illumination should be the same at all points on the section which are equidistant from the lamp (except for points in the shadow of the lamp's holder). There is also a window at the back of the box to enable the experimenter to read or write. Each window can be noiselessly opened and shut by means of a metal shutter. The space between each of the seven windows and the next also subtends an angle of 10° at O, so that the lines OS_1, OS_2, OS_3, OS_4, OS_5, OS_6 and OS_7 are inclined at 5°, 15°, 25°, 35°, 45°, 55° and 65° respectively to the central line; and similarly in the corresponding lines on the other side. Along PP' there is an aluminium diaphragm to prevent any rays of light falling on the inside of the box and being reflected through any of the windows on the screen. Along MM' and NN' are slits to admit a ground glass plate or a coloured glass plate if required for any purpose. The inside of the box is blackened. As the windows B, B', C and C' are nearer to the light than the windows A, W and A', their dimensions are made smaller in proportion so as to ensure that the illuminated rectangular area seen by the observer on the screen (mentioned below) should always be of the same size for each window.



(2) *The Nyctoptometer stand.* The box is placed on a wooden stand 18 inches high which is screwed to a table about 33 inches high.

The observer sits on a chair with the back of his head resting against the stand. The box is placed so that the cornea is directly below the light, and thus any illuminated area on the screen subtends approximately the same angle at its source as it does at the observer's eyes.

(3) *The screen.* A wire was bent to an arc of a circle whose radius was one meter and a white casement cloth was hung from it by means of rings. As the light reflected from the screen varies with the nature of the screen, it is necessary to have a standard screen as well as standard lamps in case the experiments are repeated by others. A sample of the cloth used has been kept at the Psychological Laboratory of University College.

I am indebted to Mr W. J. Jones of Messrs Siemens's Research Laboratory, Upper Thames Street, E.C. 4, for his kindness in determining the reflecting power of the screen by means of a 'lumometer.' It was found that 53 per cent. of the light incident on the screen is reflected by it[1]. We are thus in a position to measure the intensity of the threshold lights in our tests. The lamp is 1 meter distant from the screen. Hence if we multiply the candle-power of the lamp corresponding to the threshold by 0·53 we get the intensity of the minimal lights detected by the observers. The result will be in meter-candles.

The screen is attached to a wooden rod fixed on the wall and is hung at such a height that the illuminated areas fall along the median horizontal section. The screen is big enough to receive the light from the extreme windows C and C'. On the screen, opposite (but slightly below) the central window W, is a circular spot of weak red light to serve as a fixation point. A suitable light is obtained by placing a small electric lamp in a box and making a circular aperture one centimeter in diameter *above the level of the lamp* (so that the filament is invisible) and covering it with a sufficient number of red gelatine papers to make the light just visible in the dark when the box is placed immediately behind the white casement cloth. Red light is used to prevent the observer seeing the fixation point indirectly, which might be the case with lights of other colours. But for this reason a spot of luminous paint on the screen would have served as a fixation point.

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[1] Additional corroboration of this result is that Mr Le Maréchal had kindly determined the transmitting power of the screen independently by a different method at his own laboratory and found it to be 0·23. This agrees very closely with the value 0·24 obtained by Mr Jones with the lumometer.

III. METHOD OF PROCEDURE.

(1) *Visual acuity test in daylight.* A sheet of Snellen's Test Types was fixed on the wall of a room open to daylight at a suitable height for each observer. There are nine rows of black type on the sheet. The top row consisting of one capital letter can be read by an individual with satisfactory eyesight at a distance of 60 meters. We may call it the 60 meter line. The 2nd row of two letters is the 30 meter line, the 3rd of three letters is the 21 meter line, the 4th of four letters is the 15 meter line, the 5th of five letters is the 12 meter line, the 6th of six letters is the 9 meter line, the 7th of eight letters is the 6 meter line, the 8th of twelve letters is the 4·5 meter line, the 9th (or bottom row) of twelve letters is the 3 meter line.

The sheet was further illuminated by a Mazda electric lamp (B.T.H. Patent, 200 volts), placed one foot away and slightly below the card. The observer stands 6 meters away and each eye is tested. One eye is covered while the other is tested. In order to carry out this test in a scientific manner it is important to keep the conditions constant, for the estimate of acuity varies considerably according to the illumination of the test types. Yet no official regulations of any government department state the illumination of the standard, and in actual practice the illumination may vary from bright diffuse daylight to dull artificial light! Neither is there uniformity of procedure. A common method is to stop at the line where the person tested makes two mistakes. Such a procedure, however, is too greatly influenced by mere chance. In our tests credit was given for every letter read correctly no matter what its position is on the card. For the purpose of evaluating the results, Spearman's method[1] of right and wrong cases for unequal steps was adopted. By such a procedure each observer gets an exact mark representing his ability. The calculation is quickly made and the advantages are obvious since the observers can be arranged in order of merit, and it is also possible to calculate what correlation exists between an observer's visual acuity in daylight and any other function for which exact data are available.

(2) *Standard Adaptation.* To ensure that all observers are similarly adapted at the start, a high candle-power lamp is placed for 5 minutes on the stand above the observer's head. A Siemens 1000 candle-power XL lamp (220 volts, gas filled) was found suitable. During this time the observer looked on the white screen, but in order to prevent fatigue he

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[1] This *Journal*, II. January, 1908.

does not fixate a particular point. By placing the lamp in the above position there is no shadow on the screen and all parts of the retina are similarly adapted.

(3) *Main Experiment.* The 1000 candle-power lamp is extinguished and the observer is in darkness. After an interval of 2 minutes (taken to avoid irregular judgments):

(*a*) The central window *W* is opened. The resistance (see below) is decreased by tenths of an ohm until the light on the screen is visible to the observer. The observer is told beforehand that the light will appear immediately above the luminous fixation point. The window is shut after the observer says that he sees the light. The metronome is now started, beating seconds. The resistance is increased by a tenth of an ohm at a time and after each increase the observer has to judge. He is told to concentrate his attention on the screen and to fixate the luminous point after receiving the signal 'Now,' and to say when the light appears. He is told that the light will appear at any time between 1 and 11 seconds after the signal and that it will only appear for a short time (about 1 second). By this procedure the possibility of giving correct answers by mere guesswork is practically eliminated. The observer is allowed 1 second for his answer and is told to relax his attention after each judgment and to shut his eyes. When the observer has failed three times (not necessarily successive) to perceive the light, it is assumed that the threshold has been passed, *e.g.* if he fails at 3·7 ohms, 3·8 ohms and 3·9 ohms, it is assumed that the resistance corresponding to the threshold is 3·6 ohms.

(*b*) The windows *A* and *A′* are opened and the resistance decreased by tenths of an ohm until the two lights are visible to the observer who fixates the luminous fixation point. Then the same procedure as above is adopted, but, in this case only one window (either *A* or *A′*) is opened and the observer answers 'Left' or 'Right' as the case may be as soon as he sees the light. The light appears as before at any time between 1 and 11 seconds after the signal. By this procedure the observer cannot gain anything by not conscientiously fixating the luminous point.

(*c*) The same procedure is repeated with windows *B* and *B′*.

(*d*) The same procedure is repeated with windows *C* and *C′*.

(*e*) *Acuity tests for the dark-adapted eye.* For this purpose two tests were employed.

Test 1. A card was prepared containing six letters, arranged in two rows as in the accompanying sketch. The size of the type was that found in the '9 meter line' on Snellen's card. The card is placed on the screen and is illuminated by opening the central window *W* (the observer being one meter distant from the screen as in all the dark-room experiments). The initial illumination is too low for the observer to be able to read any of the letters, but the resistance is gradually reduced by a tenth of an ohm at a time, the experimenter saying 'Now' after each change, until the observer can read four letters correctly. The corresponding resistance is noted.

C T G

O E L

Test 2. A similar procedure is adopted with the test type which is found in the '3 meter line' on the Snellen card.

The whole procedure, that is (*a*), (*b*), (*c*), (*d*) and (*e*) completes the first experiment, and this is gone through three times again without a break, the observer's eyes becoming more and more dark-adapted. The time during which the observer has been kept in darkness, *i.e.* the adaptation time, is given for each reading in Table A and Table B (pp. 317, 318). The correct time for starting each experiment was noted by means of a radiolite watch. Four different cards were prepared for (*e*) *Test* 1 and similarly for (*e*) *Test* 2, namely, one for each experiment. The cards were so arranged that no letter appeared twice in the same position and so that no letter followed the same letter twice. Thus the observer was far less likely to give a correct answer by guesswork[1].

Becker's Three Dial Resistance Box was used to measure the resistance. Opposite the zero on each dial we placed a small circular piece of luminous paper. Similar pieces could also be fixed on the end of each of the three arms if necessary. The dials are hundredths, tenths and units, so that the total resistance of the box is 9·99 ohms. When each observer entered the dark room the 'compensating resistance' is noted (about 0·6 ohm). By this is meant the resistance required in the circuit in order to reduce the current passing through the lamp to 0·7 ampere, which was our starting point for all observers. This 'compensating resistance' is required owing to the fact that no accumulator can be found whose voltage remains perfectly constant. The 'compensating

[1] Letters were used in the tests for the dark-adapted eye as well as in the daylight test for visual acuity in preference to tests that are often recommended, namely Snellen's illiterate E-test, Landolt's C-test and the counting of dots. With the illiterate E-test as well as with Landolt's test there would be some difficulty in the dark owing to the observer making mistakes in his answers even when he has correctly perceived the stimulus, and it is notorious that some observers are very inaccurate in counting. Besides, there would be a probability of answering correctly by chance. This could be largely eliminated, it is true, by having a dozen possible positions for the E and the C, but in that case it would be a difficult matter for an untrained observer to judge the exact position.

resistance' is also noted after the testing of each observer is concluded. This was found generally not to vary from its initial value. If it varied slightly, then the mean of the two readings was taken as the zero for that observer. The resistance values actually obtained in the experiments were afterwards corrected by subtracting from them the value of this 'compensating resistance.' Then the resistance values were translated into current values (amperes) and eventually to illumination values (candle-powers) by obtaining a graph relating current to illumination by means of a photometer. For this purpose one lamp (No. 3) is kept as a standard and all other lamps are compared with it.

Griffin's Moving Coil Ammeter was used to measure the current. It is dead-beat in action and has a range from 0 to 1 ampere, which is divided into hundredths on white celluloid. As the amperage range required for the experiments varied between 0·50 and 0·79 amperes, it is seen that an ammeter with a fine gradation is required, for the candle-power of the lights increases rapidly with increase of current, thus,

amperes	candle-power
0·788	0·294000
0·500	0·000007

For the curve connecting candle-power and current is a logarithmic one of the form

$$x = Pa^Q,$$

where x denotes candle-power, a denotes amperes, P and Q being constants.

The current was obtained from an accumulator—Type No. 2583 (4 volts) of the Rotax Motor Accessories Co., Ltd.

IV. RESULTS.

(a) *Results of other investigators.*

Aubert found the light sensitivity after two hours' dark-adaptation to be only thirty-five times that at its beginning. His method was to use a platinum wire whose glow was just visible to the observer before dark-adaptation, then by altering the length of the wire the threshold value for any adaptation-time could be calculated. According to Nagel[1], however, the method had certain defects, one being that the state of light-adaptation, from which the observers started, could not be determined.

Nagel[1] himself, by means of his adaptometer, found the increase in sensitivity to be enormously greater than that found by Aubert, as is seen in the following table:

Adaptation (mins.)	0·5	4	9	14	19	23	26	31	39	51	61
Sensitivity	20	75	1850	10400	26300	69500	94700	174000	195000	208000	215000

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[1] von Helmholtz, *Physiologische Optik*, 1911, II.

In order to get his observers uniformly adapted, Nagel kept them out of doors in bright sunshine from 20 to 30 minutes. They were directed to look on bright objects, their brightness however not being sufficient to produce dazzling. After this preparation he states that a white surface subtending an angle of 10° at the observer's eye is just visible before dark-adaptation when illuminated by a 'normal candle' placed one meter away. Thus the sensitivity of Nagel's observers at the start is 1.

For at least three reasons the results obtained in the present paper cannot be compared directly with those of Nagel—

(1) Nagel gives no particulars concerning his observers, *e.g.* number, occupation, etc.

(2) The methods of insuring standard adaptation at the beginning were not the same.

(3) Nagel's threshold values were monocular, in the present paper they were binocular in order to conform to an aviator's actual practice, and it has been ascertained by Piper that in dark-adapted eyes binocular summation of stimuli occurs, so that the sensibility is about twice as great with both eyes as with only one, though individual variations occur[1].

It should be noted, however, that in the S tests where the vision is 60° peripheral the amount of binocular summation would be very small. This may account for the higher values obtained for the thresholds in the S tests.

After dark-adaptation of 4 minutes the average sensitivity of our observers is much greater than that found by Nagel, but after 40 minutes the reverse obtains. It should however be added that one observer L has a threshold after 40 minutes' adaptation which corresponds with that obtained by Nagel.

Further, our average threshold for Test P is higher than it otherwise would be, owing to the abnormal figures obtained for observer B.

(b) *Own Results.*

Notation. For the sake of brevity the letters P, Q, R and S refer to the tests for light sensitivity with the following windows respectively—central, 20° peripheral, 40° peripheral and 60° peripheral; the letters X and Y refer to the tests for the acuity of the dark-adapted eye with the big and small type respectively, while θ refers to the test for ordinary visual acuity in daylight. The number after each letter refers to the time in minutes for which the observer has been dark-adapted. At the end of this paper will be found tables giving the numerical results obtained. Table A gives the threshold values measured in candle-meters for each observer for tests P, Q, R and S. Table B gives the same for tests X and Y (see p. 317 *et seq.*).

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[1] Cf. J. Herbert Parsons, *Colour Vision.* Cambridge Univ. Press, 1915.

Table C gives the threshold values for each observer for test θ, as calculated by Spearman's method of right and wrong cases[1]. In this table the smaller the number, the greater the acuity; for the number refers to the size of the type which can be read at a distance of six meters.

Table D gives the correlation coefficients calculated from Tables A, B and C by the product-moments formula. Thus, for example, it is seen in the table that the correlation between $P\,4$ and $P\,40$, or $\frac{P\,4}{P\,40}$ as we may call it, is 0·25. Before considering the deductions which can be drawn from these coefficients, we may point out certain features which are already apparent in Tables A, B and C.

(1) *Individual Differences.* We are at once struck by the magnitude of certain individual differences. This is the more remarkable as all our observers belonged to a special class, namely flying officers who did not differ much in age from one another and who had all passed the standard tests for vision of the Royal Air Force. Thus confining our attention to the test for central vision, it is seen that observer E, after an adaptation of 4 minutes is only sensitive to a light of 0·00127 candle-meter, while C can see a light of 0·00009 candle-meter. C is therefore fourteen times as sensitive as E.

Again, after an adaptation of 40 minutes the values for B and L are 0·000153 candle-meter and 0·000006 candle-meter respectively. Thus L is twenty-five times as sensitive as B. Yet if we examine the figures obtained for these observers in the daylight visual test no striking differences are seen.

Again, if we calculate the mean threshold for light sensitivity for our observers for central vision after 4 minutes and after 40 minutes adaptation, namely 0·00041 candle-meter and 0·000031 candle-meter, we see that the former is over thirteen times as great as the latter. This ratio, *i.e.* the sensitivity to light after 40 minutes' compared with that after 4 minutes' adaptation, varies from a maximum value of fifty-six for one observer to a minimum of four for another. A larger number of observers would be required before standard values could be obtained for these thresholds for various periods of adaptation. But our data tend to show that if a large number of persons were tested, after an adaptation of 40 minutes there would be some individuals belonging to each of the following classes:

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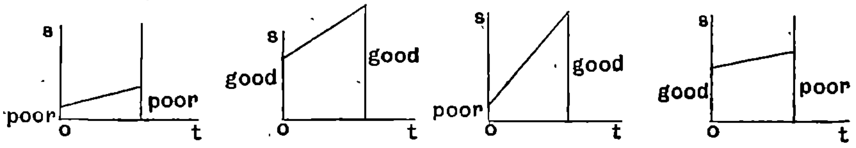
[1] This *Journal*, II. January 1908.

(1) Poor 'adapters' both at the beginning and end.
(2) Good 'adapters' both at the beginning and end.
(3) Poor 'adapters' at the beginning and good at the end.
(4) Good 'adapters' at the beginning and poor at the end.



(1) (2) (3) (4)

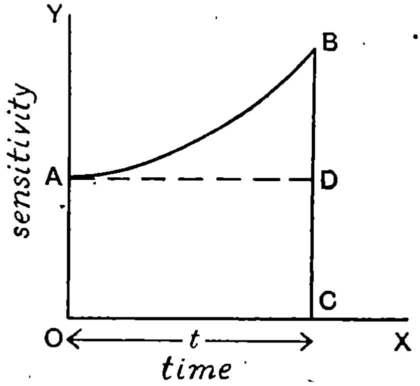
os—sensitivity. ot—time.

Further research would give the relative sizes and distribution of these classes. It is easily seen that a person's light sensitivity after an adaptation of *t* minutes can be considered to be made up of

(1) his sensitivity at the beginning of adaptation,
(2) the increase in sensitivity that has taken place during *t* minutes.

Thus in the diagram

$$CB = OA + DB.$$



So that if we find an individual's sensitivity after an adaptation of *t* minutes to have the value *CB*, this tells us nothing about its initial value, neither does it tell us what increase has taken place during the time *t*. The fullest information is given by a complete adaptation curve, but a study of the correlation coefficients in Table D will tell us whether initial ability correlates with final ability or not. It is only by such a method after testing a sufficient number of individuals that it will be possible to judge the relative importance of the threshold values at the beginning and at the end of an adaptation period.

Nagel uses the term 'Adaptionsbreite' for the ratio of the sensitivity of the eye after one hour's dark-adaptation to that of the light-adapted eye. The latter as we have seen he takes to be unity. It will be seen later that probably a more useful term for practical purposes would be the absolute value of the sensitivity after dark-adaptation of some 16 minutes.

Turning now to study the correlation coefficients given in Table D the following are the conclusions to be drawn:

(2) *Central and peripheral vision.*

(*a*) A study of the light sensitivity of our observers, as measured by each of the tests P, Q, R and S, shows that the results obtained by any two of these tests correlate highly with one another. It is true that, owing to the procedure adopted, the times of adaptation are not exactly equal in the different tests, but as regards the following coefficients the differences in time are not big enough to alter the significance of the result, thus:

$$\frac{P\,16}{Q\,18} = \cdot 86;\ \frac{P\,16}{R\,20} = \cdot 72;\ \frac{P\,16}{S\,22} = \cdot 86;\ \frac{Q\,18}{R\,20} = \cdot 75;\ \frac{Q\,18}{S\,22} = \cdot 90;\ \frac{R\,20}{S\,22} = \cdot 82.$$

$$\frac{P\,28}{Q\,30} = \cdot 85;\ \frac{P\,28}{R\,32} = \cdot 95;\ \frac{P\,28}{S\,34} = \cdot 81;\ \frac{Q\,30}{R\,32} = \cdot 90;\ \frac{Q\,30}{S\,34} = \cdot 81;\ \frac{R\,32}{S\,34} = \cdot 65.$$

$$\frac{P\,40}{Q\,42} = \cdot 85;\ \frac{P\,40}{R\,44} = \cdot 94;\ \frac{P\,40}{S\,46} = 1\cdot 0;\ \frac{Q\,42}{R\,44} = \cdot 96;\ \frac{Q\,42}{S\,46} = 1\cdot 0;\ \frac{R\,44}{S\,46} = 1\cdot 0.$$

We thus arrive at the important result that in testing for nyctopsis it is not necessary to examine both central[1] and peripheral vision as the ranking of the observers with respect to the latter would agree with that obtained with respect to the former. This means a great saving of time. Thus if our tests were repeated, it would be unnecessary with normal observers to use the peripheral windows, so that tests Q, R and S could be left out. It is, however, necessary to add that the results for central and peripheral vision would not be expected to agree for an observer with peripheral opacities of the cornea. Such a condition, however, could be detected by other means.

(*b*) If we inspect the values obtained for the average threshold for light sensibility in Table A, we see that the lowest values are obtained in Test Q that is, for 20° peripheral vision. This result would be expected in view of the researches of Breuer and Pertz[2] who showed that the peripheral rise in sensibility in dark-adaptation is rapid from 1° to 4° around the fovea, then slower to a maximum between 10° and 20° beyond which it falls. Breuer and Pertz, however, do not appear to have explored regions much beyond 20° towards the periphery, but the results obtained in the R tests (*i.e.* 40° peripheral) show that the fall in sensibility up to this point is very small (cf. average thresholds for R test in Table A). It is true that there is a greater fall when we come to the S test (*i.e.* 60° peripheral) but in this case it is probable that part of the fall is due to the thresholds approximating to monocular ones, for it has been ascertained that monocular thresholds for light sensibility are not so low as binocular.

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[1] 'Central' does not mean strictly 'foveal' in our tests, but means vision up to 10° peripherally, *i.e.* the vision obtained by using the central window W.

[2] V. Kries, *Ztsch. f. Psychol. u. Physiol. d. Sinnesorg.* 1897, xv. 327; see also J. Herbert Parsons, *Colour Vision*. Cambridge Univ. Press, 1915.

(3) *Critical time of adaptation.*

How long must an individual be kept in the dark when tested nyctoptically? Is it necessary that he should remain in the dark for an hour or will a much shorter period answer the purpose?

Duane[1] states that a period of adaptation of 10 minutes which the patient must pass with bandaged eyes in a perfectly dark room is sufficient for practical purposes for testing the light sense. Let us test this question by reference to our intercorrelations. For an adaptation of 4 minutes, we have

$$\frac{P\,4}{P\,16}=\cdot 30;\quad \frac{P\,4}{P\,28}=\cdot 23;\quad \frac{P\,4}{P\,40}=\cdot 25.$$

Thus the coefficients are all low, which shows that the results after 4 minutes do not correlate highly with those obtained with the longer periods. In other words 4 minutes is too short a time for our purpose. Similarly 6, 8 or 10 minutes fail to give us higher coefficients, thus:

$$\frac{Q\,6}{Q\,18}=\cdot 35;\quad \frac{Q\,6}{Q\,30}=-\cdot 20;\quad \frac{Q\,6}{Q\,42}=-\cdot 22.$$

$$\frac{R\,8}{R\,20}=\cdot 08;\quad \frac{R\,8}{R\,32}=-\cdot 17;\quad \frac{R\,8}{R\,44}=-\cdot 16.$$

$$\frac{S\,10}{S\,22}=-\cdot 08;\quad \frac{S\,10}{S\,34}=-\cdot 13;\quad \frac{S\,10}{S\,46}=-\cdot 24.$$

The P, Q, R and S tests yield no figures for 12 or 14 minutes. For these times we have the X and Y tests only. Here the coefficients are distinctly higher namely:

$$\frac{X\,12}{X\,24}=1\cdot 00;\quad \frac{X\,12}{X\,36}=\cdot 58;\quad \frac{X\,12}{X\,48}=\cdot 59.$$

$$\frac{Y\,14}{Y\,26}=\cdot 80;\quad \frac{Y\,14}{Y\,38}=\cdot 73;\quad \frac{Y\,14}{Y\,50}=\cdot 67.$$

For an adaptation of 16 minutes or longer, we do, however, get a still higher average of coefficients, thus:

$$\frac{P\,16}{P\,28}=\cdot 94;\quad \frac{P\,16}{P\,40}=\cdot 86$$

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[1] Fuchs's *Textbook of Ophthalmology*, transl. by Duane, 1919, 127.

(the only exceptions being $\frac{R\,20}{R\,32} = \cdot 68$ and $\frac{R\,20}{R\,44} = \cdot 60$ which are not so high). We may therefore conclude that an adaptation of 16 minutes is sufficient for practical purposes.

This result agrees very well with Mr Flügel's estimate[1] of 20 minutes, especially when we bear in mind his statement that the anomalous behaviour of four of his observers has lowered some of the correlations.

(4) *Visual acuity in daylight* (θ) *does not correlate significantly with light sensitivity at any adaptation time.*

This is shown by the following coefficients:

$$\frac{P\,4}{\theta} = -0\cdot 10; \quad \frac{P\,16}{\theta} = -0\cdot 16; \quad \frac{P\,28}{\theta} = -0\cdot 10; \quad \frac{P\,40}{\theta} = -0\cdot 14.$$

$$\frac{Q\,6}{\theta} = -0\cdot 22; \quad \frac{Q\,18}{\theta} = -0\cdot 26; \quad \frac{Q\,30}{\theta} = -0\cdot 33; \quad \frac{Q\,42}{\theta} = -0\cdot 23.$$

$$\frac{R\,8}{\theta} = -0\cdot 04; \quad \frac{R\,20}{\theta} = -0\cdot 08; \quad \frac{R\,32}{\theta} = -0\cdot 22; \quad \frac{R\,44}{\theta} = -0\cdot 12.$$

$$\frac{S\,10}{\theta} = 0\cdot 02; \quad \frac{S\,22}{\theta} = -0\cdot 12; \quad \frac{S\,34}{\theta} = -0\cdot 26; \quad \frac{S\,46}{\theta} = -0\cdot 35.$$

In view of the fact that the above correlations all, except one, are negative, one might incline to the view that there is a slight negative correlation between the two functions. Such a result would have some theoretical bearing on the duplicity theory. But the deduction would be unwarranted without further research, as the coefficients are not big enough to be significant, even the highest, $-0\cdot 35$, being only about twice the size of its probable error ($0\cdot 17$). The deduction warranted by the figures is therefore as stated, and this agrees with Mr Flügel's result obtained by a different method.

(5) *Visual acuity in daylight* (θ) *correlates moderately with the visual acuity in a dim light.*

This is shown by the following coefficients, all of which, except the first, exceed three times the size of their probable error.

$$\frac{X\,12}{\theta} = \cdot 40; \quad \frac{X\,24}{\theta} = \cdot 57; \quad \frac{X\,36}{\theta} = \cdot 69; \quad \frac{X\,48}{\theta} = \cdot 72.$$

$$\frac{Y\,14}{\theta} = \cdot 49; \quad \frac{Y\,26}{\theta} = \cdot 71; \quad \frac{Y\,38}{\theta} = \cdot 78; \quad \frac{Y\,50}{\theta} = \cdot 77.$$

This result agrees with that obtained by Mr Flügel, although there is some divergence in the magnitudes of the coefficients.

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[1] This *Journal*, 1921, XI. pp. 289–298.

(6) *Light sensitivity does not correlate significantly with the visual acuity in dim light.*

This is best seen by inspecting the intercorrelations between the P's and the X's and also between the P's and the Y's in the top right-hand corner of Table D. Mr Flügel[1], however, finds a slight positive correlation between the two functions. From the practical point of view, nevertheless, both results show that no inference can be drawn from an individual's performance in X or Y as to his ability in P or *vice versa*. It might be pointed out that Mr Flügel's X and Y tests are not directly comparable with those of the present writer. In the former the figures were varied and the light was kept constant, in the latter the figures (letters) were kept constant and the light was variable. Moreover, the intensities of the lights differed in the two procedures. Mr Flügel's intensities were roughly 0·1 and 0·2 candle-power in tests X and Y respectively. In those of the present writer the lights were much dimmer. The mean thresholds in the X and Y tests after 48 minutes' and 50 minutes' adaptation were 0·0037 candle-power and 0·043 candle-power respectively. One would therefore expect, *a priori*, that the correlation between P and X in our experiments would be higher than in Mr Flügel's, for one would assume that P would be the more involved in X, the darker X was. The reverse, however, was found to be the case. This paradoxical result is in harmony with a similarly paradoxical result in Mr Flügel's experiments, as the correlation between P and Y was found greater than that between P and X. Moreover, in the experiments of the present writer the correlation between P and X is slightly less than that between P and Y, though not significantly so. I am indebted to Mr Flügel for pointing out these paradoxes. It is evident that the matter requires further investigation.

(7) *Visual acuity in the dim light* (X) *correlates rather highly with that in the much dimmer light* (Y).

This is seen from an inspection of the intercorrelations of the X's with the Y's. It will be noticed that these are higher than those obtained by Mr Flügel. One would expect this, *a priori*, in view of the fact mentioned in the previous paragraph, namely, that the lights in the present tests were much dimmer than in Mr Flügel's tests. Consequently we may surmise that the mechanisms of daylight vision will thus only have a small effect in the case of both X and Y, and that the intercorrelations between X's and Y's will therefore be rather high.

Finally the writer would lay stress on the general regularity of the coefficients which form the bases for each of the above conclusions. This regularity entitles the results to serious consideration in spite of their being obtained from such few numbers.

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[1] *Loc. cit.*

V. SUMMARY OF RESULTS.

1. Individuals differ much in light sensitivity. This holds for adaptation periods ranging from 4 minutes (where one observer was fourteen times as sensitive as another) to 40 minutes (where one was twenty-five times as sensitive as another). After an adaptation of 16 minutes the keenest observer in Test *P* could detect a light of 0·000009 candle-meter. His sensitivity therefore is $\frac{1}{0\cdot000009}$ or 111,111 units. On the other hand the sensitivity of the least sensitive observer was only 6536 units.

2. The testing for light sensitivity can be confined to a region of 10° round the fovea, as the results for 20°, 40° or 60° to the periphery give approximately the same ranking.

3. Light sensitivity attains its maximum for vision in the neighbourhood of 20° to the temporal side of the fovea.

4. Dark-adaptation for 16 to 20 minutes is sufficient for practical purposes for testing both light sensitivity and the visual acuity of the dark-adapted eye.

5. An individual's visual acuity in daylight is no criterion of his light sensitivity.

6. Visual acuity in daylight correlates moderately with the visual acuity in the dim lights used in tests *X* and *Y*.

7. No inference can be drawn from testing an individual's light sensitivity as to his visual acuity in dim light or *vice versa*.

8. Visual acuity in the dim light (*X*) correlates rather highly with that in the much dimmer light (*Y*).

VI. TABLES A—D.

Table A.

	Central vision *P* Thresholds in meter-candles				20° peripheral vision *Q* Thresholds in meter-candles			
Observer	After 4 mins.	After 16 mins.	After 28 mins.	After 40 mins.	After 6 mins.	After 18 mins.	After 30 mins.	After 42 mins.
A	·000153	·000048	·000026	·000021	·000064	·000026	·000021	·000023
B	·000636	·000153	·000153	·000153	·000153	·000090	·000064	·000064
C	·000090	·000021	·000025	·000021	·000053	·000017	·000017	·000017
D	·000477	·000025	·000023	·000023	·000200	·000023	·000021	·000021
E	·001270	·000053	·000025	·000023	·000425	·000048	·000023	·000021
F	·000477	·000026	·000023	·000023	·000262	·000064	·000023	·000025
G	·000200	·000053	·000026	·000026	·000064	·000053	·000023	·000048
H	·000262	·000042	·000024	·000021	·000010	·000042	·000042	·000025
J	·000334	·000064	·000026	·000023	·000477	·000064	·000023	·000017
K	·000425	·000026	·000012	·000021	·000200	·000026	·000021	·000021
L	·000318	·000009	·000006	·000006	·000064	·000006	·000006	·000006
M	·000318	·000017	·000017	·000012	·000334	·000017	·000006	·000006
Mean	·000410	·000045	·000032	·000031	·000200	·000040	·000024	·000025

	40° peripheral vision *R* Thresholds in meter-candles				60° peripheral vision *S* Thresholds in meter-candles			
Observer	After 8 mins.	After 20 mins.	After 32 mins.	After 44 mins.	After 10 mins.	After 22 mins.	After 34 mins.	After 46 mins.
A	·000025	·000048	·000025	·000023	·000048	·000048	·000023	·000023
B	·000010	·000053	·000090	·000090	·000153	·000090	·000090	·000090
C	·000023	·000021	·000021	·000021	·000090	·000021	·000025	·000025
D	·000262	·000025	·000025	·000023	·000318	·000048	·000048	·000048
E	·000262	·000053	·000026	·000023	·000334	·000064	·000048	·000025
F	·000064	·000026	·000025	·000025	·000064	·000053	·000053	·000053
G	·000010	·000048	·000048	·000048	·000090	·000053	·000064	·000064
H	·000090	·000050	·000050	·000026	·000090	·000050	·000042	·000025
J	·000318	·000048	·000023	·000017	·000318	·000053	·000026	·000017
K	·000153	·000026	·000021	·000017	·000200	·000025	·000023	·000021
L	·000021	·000004	·000004	·000003	·000026	·000004	·000006	·000006
M	·000425	·000021	·000017	·000017	·000795	·000023	·000021	·000017
Mean	·000154	·000035	·000032	·000028	·000211	·000045	·000040	·000035

NOTE. As the reflecting power of the screen is ·53, the above values when divided by ·53 will give the corresponding candle-power of the lamp.

Table B.

	Test *X* Thresholds in meter-candles				Test *Y* Thresholds in meter-candles			
Observer	After 12 mins.	After 24 mins.	After 36 mins.	After 48 mins.	After 14 mins.	After 26 mins.	After 38 mins.	After 50 mins.
A	·0151	·0095	·0106	·0106	·156	·156	·156	·156
B	·0035	·0033	·0033	·0022	·087	·051	·029	·025
C	·0023	·0020	·0028	·0023	·025	·029	·025	·025
D	·0035	·0033	·0035	·0035	·061	·051	·061	·051
E	·0069	·0047	·0042	·0035	·051	·035	·029	·029
F	·0033	·0023	·0028	·0023	·035	·041	·029	·029
G	·0042	·0035	·0035	·0035	·041	·030	·029	·029
H	·0085	·0037	·0033	·0028	·051	·032	·027	·017
J	·0069	·0058	·0042	·0035	·041	·035	·029	·025
K	·0069	·0035	·0033	·0022	·087	·088	·041	·029
L	·0047	·0033	·0042	·0042	·035	·035	·051	·051
M	·0151	·0028	·0028	·0028	·156	·061	·061	·051
Mean	·0067	·0040	·0040	·0037	·069	·054	·048	·043

Note. As the reflecting power of the screen is ·53, the above values when divided by ·53 will give the candle-power of the lamp in each case.

Table C.

Visual acuity in daylight, measured by Snellen's Test Types.

	Thresholds	
Observer	Right eye	Left eye
A	7·50	15·50
B	3·25	4·13
C	3·00	3·12
D	5·60	4·00
E	4·00	4·12
F	3·12	3·50
G	4·12	4·25
H	2·25	2·37
J	3·00	4·37
K	3·00	2·75
L	4·25	4·12
M	4·25	4·25

Note. Values obtained by Spearman's method of right and wrong cases. The numbers refer to the size of the type which can be read at a distance of six meters. Thus with the exception of observer A, all observers have 'supernormal' vision. In this particular case it was found advisable to use the values for the right eye in correlational work.

Table D.

	θ	P 4	P 16	P 28	P 40	Q 6	Q 18	Q 30	Q 42	R 8	R 20	R 32	R 44	S 10	S 22	S 34	S 46	X 12	X 24	X 36	X 48	Y 14	Y 26	Y 38	Y 50
θ	.	—·10	—·16	—·10	—·14	—·22	—·26	—·33	—·23	—·04	—·08	—·22	—·12	·02	—·12	—·26	—·35	·40	·57	·69	·72	·49	·71	·78	·77
P 4	—·10	.	·30	·23	·25	·63	.	.	.	·35	.	.	.	·23	.	.	.	—·22	—·05	—·16	—·22	—·11	—·21	—·16	—·06
P 16	—·16	·30	.	·94	·86	.	·86	.	.	.	·72	.	.	.	·86	.	.	—·14	·16	·05	—·05	·09	·04	—·14	—·12
P 28	—·10	·23	·94	.	1·00	.	.	·85	.	.	.	·95	.	.	.	·81	.	—·23	—·05	—·08	—·16	·12	·01	—·15	—·14
P 40	—·14	·25	·86	1·00	.	.	.	.	·85	.	.	.	·94	.	.	.	1·00	—·27	—·09	—·11	—·20	·10	—·01	—·18	—·18
Q 6	—·22	·63	.	.	.	.	·35	—·20	—·22	·65	.	.	.	·63	.	.	.	·15	.	.	.	·02	.	.	.
Q 18	—·26	.	·86	.	.	·35	.	·81	·82	.	·75	.	.	.	·90	.	.	.	·01	.	.	.	—·12	.	.
Q 30	—·33	.	.	·85	.	—·20	·81	.	1·00	.	.	·90	.	.	.	·81	.	.	.	—·08	.	.	.	—·24	.
Q 42	—·23	.	.	.	·85	.	·82	1·00	.	.	.	.	·96	.	.	.	1·00	.	.	.	—·14	.	.	.	—·19
R 8	—·04	·35	.	.	.	·65	.	.	.	.	·08	—·17	—·16	·93	.	.	.	·35	.	.	.	·34	.	.	.
R 20	—·08	.	·72	.	.	.	·75	.	.	·08	.	·68	·60	.	·82	.	.	.	·49	.	.	.	·15	.	.
R 32	—·22	.	.	·95	.	.	.	·90	.	—·17	·68	.	1·00	.	.	·65	.	.	.	—·13	.	.	.	—·24	.
R 44	—·12	.	.	.	·94	.	.	.	·96	—·16	·60	1·00	.	.	.	.	1·00	.	.	.	—·13	.	.	.	—·08
S 10	·02	·23	.	.	.	·63	.	.	.	·93	.	.	.	.	—·08	—·13	—·24	·48	.	.	.	·39	.	.	.
S 22	—·12	.	·86	.	.	.	·90	.	.	.	·82	.	.	—·08	.	·87	1·00	.	·17	.	.	.	·03	.	.
S 34	—·26	.	.	·81	.	.	.	·81	.	.	.	·65	.	—·13	·87	.	1·00	.	.	—·24	.	.	.	—·33	.
S 46	—·35	.	.	.	1·00	.	.	.	1·00	.	.	.	1·00	—·24	1·00	1·00	.	.	.	.	—·31	.	.	.	—·28
X 12	·40	—·22	—·14	—·23	—·27	·15	.	.	.	·35	.	.	.	·48	.	.	.	.	1·00	·58	·59	·86	·70	·69	·95
X 24	·57	—·05	·16	—·05	—·09	.	·01	.	.	.	·49	.	.	.	·17	.	.	1·00	.	·94	·90	·50	·80	·78	·79
X 36	·69	—·16	·05	—·08	—·11	.	.	—·08	.	.	.	—·13	.	.	.	—·24	.	·58	·94	.	1·00	·52	·86	·90	·93
X 48	·72	—·22	—·05	—·16	—·20	.	.	.	—·14	.	.	.	—·13	.	.	.	—·31	·59	·90	1·00	.	·52	·87	·93	·95
Y 14	·49	—·11	·09	·12	·10	·02	.	.	.	·34	.	.	.	·39	.	.	.	·86	·50	·52	·52	.	·80	·73	·67
Y 26	·71	—·21	·04	·01	—·01	.	—·12	.	.	.	·15	.	.	.	·03	.	.	·70	·80	·86	·87	·80	.	·96	·94
Y 38	·78	—·16	—·14	—·15	—·18	.	.	—·24	.	.	.	—·24	.	.	.	—·33	.	·69	·78	·90	·93	·73	·96	.	1·00
Y 50	·77	—·06	—·12	—·14	—·18	.	.	.	—·19	.	.	.	—·08	.	.	.	—·28	·95	·79	·93	·95	·67	·94	1·00	.

NOTE. All correlations in this table which are greater than 0·34 are greater than twice their probable error.
All correlations in this table which are greater than 0·46 are greater than three times their probable error.

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THE FUNCTIONS OF IMAGES[1].

By F. C. BARTLETT.

(*From the Cambridge Psychological Laboratory.*)

§ 1. *Description of the experiments* (pp. 320–322).
§ 2. *The experimental results* (pp. 322–330).
- I. *The general influence upon sensory imagery of affective factors.*
- II. *The main characteristics of direct sensory imagery.*
  - (*a*) *Visualisation and confidence.*
  - (*b*) *The clearness of the visual image.*
  - (*c*) *Visualisation and an order of sequence.*
  - (*d*) *Changes in visual images.*
- III. *How the use of words affects direct sensory imagery.*
  - (*a*) *Vocalisation and confidence.*
  - (*b*) *Vocalisation and clearness in reproduction.*
  - (*c*) *Vocalisation and an order of sequence.*
  - (*d*) *Vocalisation and changes in reproduction.*

§ 3. *Theoretical considerations* (pp. 330–337).
  - (*a*) *The most primitive type of sensory imagery.*
  - (*b*) *The image as an aid to a checked reaction.*
  - (*c*) *The image as an alternative reaction.*
  - (*d*) *Why words tend to be substituted for sensory imagery.*

§ 1. DESCRIPTION OF THE EXPERIMENTS.

THIS paper may be regarded as a continuation of an already reported "Experimental Study of some Problems of Perceiving and Imaging[2]." One of the conclusions of that study was that the content of acts of imaging is continually undergoing change, and that the specific nature and conditions of the change call for detailed investigation. In the course of succeeding experiments, the main purpose of which was to determine the general types of transformation of images, certain suggestions as to the functions of images arose, and in the present paper I propose to formulate and discuss these suggestions.

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[1] A paper (here modified) read at a meeting of the British Association (Psychology Sub-section), Cardiff, 26 August, 1920.

[2] See this *Journal*, VIII. 1916, pp. 222–66.

The experiments themselves fall into two main groups. The material used in the first group consisted of a number of objects so much alike that they could be readily related, but at the same time possessing definite individual characteristics. This material was presented for observation under specified conditions. Afterwards it was removed, and the subjects gave a description of what they had perceived, and answered questions relating to the objects presented. The materials used consisted of five picture-postcards, each bearing a representation of a face of a military or naval officer or man. Faces were chosen because they commonly arouse interest, and the faces of army or navy men because the experiments were conducted during the early days of the late war, when interest in members of the Services was more than ordinarily widespread and intense. Moreover the pictures, while they contained comparatively little detail, and were clearly related, had each a strongly marked individual character. The cards were placed face downwards on a table, always in the same order. The instructions were:

> On these picture postcards there are a number of representations of men's faces, one on each card. I want you to look at each card for ten seconds, noting carefully all the characteristics of the face, so that later on you may be able to describe them, and to answer questions concerning them. When I say "Now" take up the uppermost card, and look at it till I say "Now" again; then put it face downwards on the table and take up the second, looking at that also till I say "Now." Treat the other cards in the same way.

The first descriptions of the cards were given, and questions answered, after an interval of 30 minutes from the observation of the last of the five faces. After a week a second description was given, again after a fortnight, and, whenever possible, again after a much longer period. The cards were not exposed for observation more than once.

Nineteen subjects were examined. Of these 9 were women and 10 men, and all were of adult age.

In a second group of experiments the material to be reproduced consisted of picture signs with which certain words were associated. There were three different series of signs, many of them taken directly from pictographic writing. Signs and words were written upon plain cards, each associated pair having a separate card. A series of cards was placed face downwards on a table. The instructions were:

> On these cards are a number of signs for words. I want you to learn to associate the sign with the word, so that afterwards if you were given the word you could reproduce the appropriate sign. You may learn the signs in any way and in any order

that you please. You will have ample time to learn them but if you think you have established the associations before I tell you to stop, let me know and I will take the cards away.

At the completion of the learning period, after an interval of fifteen minutes, the subject was told:

Now I am going to give you a piece of dictation, in which some of the words that had signs will be used. Whenever a sign-word comes, write the sign for it. Work as quickly as possible.

After the lapse of a fortnight a further piece of dictation was given, again after the lapse of another fortnight, and later still whenever possible. The actual signs were exposed for observation once only.

This 'method of picture writing,' as it may be called, was adopted, among other reasons, because the definite demand for a specific description was often taken to mean that a subject was being directly required to attempt to evoke a visual image. It was thought that the substitution of graphic representations, effected in the course of rapid writing, for detailed and deliberate description might leave a subject more free to adopt his ordinary methods of reproduction. And this was, in fact, what occurred.

For this group of experiments 17 subjects were employed, of whom 8 were women and 9 were men. As before, they were all of adult age.

In describing the experimental results obtained I propose to confine attention strictly to those that are relevant to the purpose of the present paper. Consequently no attempt will be made to analyse the results in very great detail. For the same reason it appears unnecessary to enter more fully into the exact characters of the material and methods employed. Sufficient has been said to render clear enough the nature of the situations which gave rise to the suggestions about to be advanced.

It will be seen that the paper deals directly with 'reproductive' images only.

§ 2. THE EXPERIMENTAL RESULTS.

I. The General Influence upon Sensory Imagery of Affective Factors.

As was expected, the post-card representations of soldiers and sailors aroused in nearly all subjects very considerable interest, and their perception was accompanied by no small amount of affective tone. This feeling, and the attitudes to which it was attached—of acceptance, satisfaction, dislike, disapproval, and so on—commonly assumed an important *rôle* in determining the process of recall. As a rule, the affective tone clustered about the 'general impression' of a face. It appeared as soon as a card was exposed to observation, and persisted throughout the course of the experiment. For example, a subject described one face as that of a "very serious-looking young man." This was a 'general impression,' as she said, not based upon any definite analysis of the picture, but in so far as it was attached to any particular feature, attributed vaguely to some effect of the chin. When the first description was called for, the affective accompaniment persisted or recurred. The face was characterized as 'serious,' and 'very determined looking.' The impression was, though indefinitely, set down to the chin, and accordingly the face was—wrongly—turned into complete profile. But the definite turning of the face into profile was rather the result of an attempt to account for a 'general impression' of seriousness, than a direct sensory image evoked immediately through the persistence of the affective accompaniment. After the lapse of three weeks the impression of seriousness had become greatly intensified: "This is the young man in profile to the right. He had a square face, and is very serious and determined looking." What occurred in this instance was that the feeling aroused by the original perception of the face persisted or recurred. With it came, in vaguest possible terms of sensory imagery, an 'impression' of a serious countenance. The seriousness was, equally vaguely, connected with the lower part of the face, and so in the end a definite square chin seen in complete profile was evolved.

In general, it appears certain that the image which is reinstated mainly by the aid of affective cues tends to be of a vague and unanalysed character.

"I think," said one subject, "that this man was a sailor, but I'm not sure about him. He has almost completely faded out. That's all I can say. I have no picture of the man....I've a very vague impression of a rather jolly face."

When the whole of the experiment was over this subject saw the card in question again.

"That's very curious," he said. "About that card, I had the vaguest possible impression of a genial countenance. It was a sort of undefined and shifting frame, but the real face would fit into it very well. It seemed all the while to hover on the brink of being an image. It was not a visual image, and yet I think it came near to being one; and perhaps it was only the extreme indefiniteness that stood in the way."

The whole bearing and evidence of this subject showed that his behaviour in this case was very largely affective.

"What I seemed to be going upon chiefly," said another subject, "was the general impression. You see a thing, and that creates an atmosphere. Then the atmosphere seems to cling on, or to come up again, and you get an image of the thing."

One observer gave four sittings with the picture-sign material. At first she relied mainly upon direct cues which recurred in visual form. In the subsequent tests she showed herself distinctly more sensitive to affective factors. On the final occasion, after an interval, from the latest test, of exactly four months, she strongly maintained that absolutely *all* the reproductions she effected were now carried out by the help of a peculiar 'feeling,' which she could not properly analyse but which appeared to be a compound of affective and motor constituents. The reproductions now became characteristically vague, losing their individuality of form. Where several different signs were said to give rise to the same general feeling-tone, confusion of the signs in question practically always occurred.

In sum, the suggestion is clear, that the function of affection in reproduction was to reinstate a *situation* rather than a specific object, and consequently that the sensory imagery to which it might lead tended to be vague, schematic and relatively undifferentiated.

II. THE MAIN CHARACTERISTICS OF DIRECT SENSORY IMAGERY.

In what follows I shall take it that visual imagery is typical of direct sensory imagery of all kinds. Allowing, that is, for differences in mode, it will be assumed that visual reproduction serves the same functions as reproduction in terms of hearing, of smell, or of any other sense.

(a) *Visualisation and confidence.*

Throughout the whole of the experiments, as consistently as is ever the case in psychology, the occurrence of definite visual imagery tended to set up an attitude of confidence in the accuracy of the reproductions. For example, a subject who was not normally a good visualiser, had described one of the cards already referred to in a halting manner. He was then asked a question, and said: "That question brought back an image." He described the face minutely, adding: "It became distinct when you asked the question, and that was the first time I really had an image of the card." Another subject had returned a most indecisive series of answers to questions concerning some of the special characteristics of a face. Suddenly his whole demeanour changed. He became confident and said: "I had an image just then."

It was not only in the card series that this confident attitude was marked. With totally different material, and with different subjects, the picture-signs brought out exactly the same characteristic. Marked visualisers throughout worked rapidly and with the utmost confidence, although, as a matter of fact their reproductions were frequently erroneous.

(b) *The clearness of the visual image.*

While the general feeling-tone attaching to a whole situation undoubtedly facilitated its reproduction in crude image form, the actual clearness of the image usually seemed to be traceable to some 'striking' detail[1]. This appeared most distinctly from the results of the postcard series. For in this set of experiments it was possible to get definite information concerning the relative clearness of the different faces in the series for a given subject. If we allow for the well-known superiority of the first-presented member of a series in respect of the clearness of its reproduction, we shall find that in 75 per cent. of the total number of reproductions those of one face were clearer than those of any of the others. This face is marked by a large and noticeable moustache, and to the moustache the clearness of the reproduction was practically always attributed.

Thus if an object or situation as a whole merely gives rise to a general pleasing affect, while we are likely to recur to it readily, we are not likely to be able to reproduce it very definitely. But when some detail of a presented object is unusually outstanding, the other features are readily grouped around this detail, and the whole image tends to acquire a distinct and well-defined character.

The conditions which make any particular detail striking call for careful additional investigation. We gain nothing of psychological importance by saying that the 'striking' detail is one which appears 'comic,' or 'out of the way,' or 'pleasing.' For we are then merely left with the further question as to what types of detail do so appear, and why they produce their effect.

Every observer brings to his task of interpreting and responding to a presented situation more or less well-marked tendencies to behaviour and to affectivity. Some of these are general and instinctive; others are personal, and belong to the nature of individual interests or temperamental differences. Certain details in presented situations excite these tendencies more readily than do others. These constitute, probably, the most numerous class of the details which are designated 'striking,' and it is upon them that the formation of clear and distinct images in the main depends.

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[1] Cf. J. Philippe: "Qu'est-ce qu'une image mentale?" *Rev. philos.* 1902, LIV. 49–51.

(c) *Visualisation and an order of sequence.*

If we consider the postcard experiments, some very interesting suggestions arise with regard to the capability of sensory imagery for dealing with 'orders of sequence.'

Every one of the errors made in reporting on the order in which the faces had been presented was made by subjects who appeared to be relying mainly upon a visual mode of reproduction. Of the 19 subjects, 12 were dominantly of visual, and the other 7 more or less clearly of 'vocal' type; 8 of the 12 visualisers made mistakes as to the order of presentation, but none of the vocalisers.

This leads us at once to suspect that the difficulty of retaining an order of sequence is connected with the oft-noted characteristic individuality of the sensory image. The members of the series presented in succession are, that is to say, apprehended by the visualisers in relative isolation. General characters such as a law of order or of successive relation cannot be directly and conveniently dealt with by sensory imagery, owing to its dominantly concrete character (cf. pp. 329, 331 *et seq.*).

Again the very fact that in a succession of images the different members may display very great divergencies as to clearness, may itself lead to difficulty in the reproduction of order. For perhaps the members most clearly reproduced tend to get thrust forward into an early place in the series.

(d) *Changes in visual images.*

The most definite evidence as to changes introduced into successive reproductions is to be drawn from the picture-sign experiments. The total number of test words dealt with by my subjects was 920. The main transformations that occurred may be comprised under the heads of: omissions, reversals, blending, substitution, simplification and elaboration, and inventions. In all they give 470 instances of change, and when it is remembered that of 53 cases of blending, each involves at least two signs, while some involve three signs, it will be clear that changes affecting the reproduction of material of this nature are very frequent indeed.

An analysis of the modes and conditions of change is of great interest, but would lead away from the subject of the present paper. It is of direct interest, however, that visual images are affected by such transformations to a very great extent. For example one of the best of the visualisers introduced changes into 80 per cent. of the signs with which he dealt, and just over 50 per cent. of these transformations occurred after an interval of more than a week had elapsed since the actual observation.

This result may be fairly regarded as representative of what commonly occurred. That is, if reproductions are obtained in terms of sensory imagery at increasing intervals of time, transformations are unwittingly effected. Processes of change may continue for long periods and in fact for as long as the image may be evoked. The transformations may be of all kinds, but it is of interest to note that all of the subjects who showed any marked tendency towards inventions were adopting the plan of relying upon visual imagery. The presumption is, in fact, very strong that invention, the bringing in of totally new details, is particularly prone to occur in the course of the use of sensory imagery.

III. HOW THE USE OF WORDS AFFECTS DIRECT SENSORY IMAGERY.

There is one medium of reproduction which may be used with equal suitability, whatever the mode of sensory experience to which the object was originally presented. We cannot, in any strict sense of the word, visualise a taste or a smell or a sound, but we can name them all equally well. Thus all those situations which often evoke sensory imagery may be dealt with by the help of words, and words can equally well be employed in other cases also. It is a matter of common observation that many persons, if they have occasion to recur in mind to some object or situation which has been presented visually, do so by finding a series of names for the whole and its parts, without the least resort to visualisation. The words, in this case, act in the place of the sensory images; and, in much the same way, sensory images may, on other occasions, be said to act in the place of words. Very often both are used, and it is in such cases especially that sensory imagery is apt to be of a shortened and fragmentary form.

Now in dealing with the relation between the use of words and direct sensory imagery the distinction between what may be called verbalisation and vocalisation is important. The term verbalisation simply indicates the 'employment of words.' But the mere use of words may not of itself displace sensory imagery. For words may be imaged just as well as any other objects, and may appear in visual or auditory forms. But in vocalisation, which is a special type of verbalisation, the words employed are produced in external or in internal speech; and it is perfectly possible for them to be employed entirely apart from any sensory imagery of the object to which the word refers or, in some instances, of the words themselves. This situation in fact was of very common occurrence in the experiments with which the present paper is concerned.

Further, it is apparent that words may be strung together and spoken by mere habit. There are word-movement habits, just as definitely as there are hand- or leg-movement habits, as the every-day conversation of most people will very well show. But this mere habitual repetition of phrases is to be distinguished from vocalisation proper.

Accordingly in what follows I propose to employ the term 'vocalisation' to indicate the use of words as a means of reproduction, when neither the words nor the objects to which the words refer are necessarily present in any form of sensory imagery; and further when the words are not merely strung together so as to form an automatic series. Such vocalisation affects sensory imagery in ways which it is important to discuss.

(*a*) *Vocalisation and confidence.*

As definitely as the visualising subjects were confident the vocalising subjects tended to be doubtful. They were prone to enter into long explanations and justifications of their attempted reproductions. An extract from my notes, typical of the behaviour of these subjects, may be given:

> A fortnight later (*i.e.* after the first exposure of the signs) there was a further test. Subject F hesitated at the word 'broad.' He was very uncertain, and said, "Oh, this is hopeless. No; I can't think of anything. But there may have been a sign." At nearly every word he continued to pause, saying: "Now let me see, have I had a sign for that?"

Similar remarks are made concerning the procedure of all of the vocalising subjects, and the doubtful attitude adopted was so constant and so marked as to make it appear to me exceedingly unlikely that the diminution of confidence was unconnected with the increased reliance upon words.

(*b*) *Vocalisation and clearness in reproduction.*

General affective factors and 'striking' details had precisely the same effects as in the case of visualisation, nor did they appear to be less commonly operative. But while the subjects who relied upon sensory imagery, as a rule, adopted a method of direct evocation of the reproduction required, the vocalisers all relied to a far greater degree upon indirect reinstatement through associations. The common procedure was that when a sign was first exposed the subject remarked—usually aloud—"that reminds me of so-and-so," or "that is like so-and-so." When the time for the reproduction arrived, first the phrase employed or the name for the associated object would be given, and then the desired sign. The associations used were very frequently indeed suggested by some detail of a sign, rather than by its form or meaning as a whole. Thus the use of words tended to set up a more analytic attitude, and as a consequence 'striking' detail tended to be somewhat more often noticed.

(c) *Vocalisation and an order of sequence.*

It has already been stated (see p. 326) that the vocalising subject attains a distinctly superior position as regards accuracy in the reproduction of an order of sequence. In the postcard series absolutely no mistake was made as to order of presentation by a subject who relied mainly upon words. This was connected with the tendency on the part of the vocalisers to analyse whatever was presented for their observation. Once an order of arrangement was definitely attended to, words became a great help. For serial relations can be named as definitely as concrete objects. On a first examination of the postcards the subject would say: "*First* A, *second* B, *third* C," and so on, and the order would thus be at least as definitely fixed as the nature of the members which made up the series. In subsequent reproduction therefore, the order was preserved correctly; for the analysing attitude induced by the use of words directs attention upon the ways in which the members of an order are related one to another; and once the relations in question have been discriminated, words form entirely suitable instruments by which they may be retained.

(d) *Vocalisation and changes in reproduction.*

Reproductions effected by the aid of the use of words were just as subject to transformation as were those expressed in direct sensory imagery. Moreover, the changes were of an exactly parallel character, and continued over as long a period. But in one important respect there was a difference. While the visualiser with his direct individualising tendencies, was rather more inclined to invent new details, the vocaliser, with his bent towards analysis, was prone to blend and confuse the old details. Both with the postcards and with the picture-signs the vocalisers tended to group together some, or all of the members of a series and to formulate some law of their relation. Twice, for example, in the postcard series, subjects who relied upon words formulated a 'law' as to the 'direction of regard' of the different faces. They averred that "the successive faces turned gradually from complete left profile to complete right profile." The influence of such formulated 'laws,' which were generally too simple to meet the actual facts, definitely increased with lapse of time, and often led to a transference of detail from one card and its blending with that of another. To this, largely, was due the greater tendency on the part of the vocaliser to condensation or blending.

§ 3. THEORETICAL CONSIDERATIONS.

(a) *The most primitive type of sensory imagery.*

Upon the basis of the experimental results which have been briefly described, I propose now to offer some suggestions as to the parts played by images, and by the use of words, in the psychological processes of reproduction.

The first question is as to the most general circumstances in which a sensory image is evolved in order to facilitate a reference to some object or situation not immediately present. In all experiments the image is called up under the direct influence of a 'demand for reproduction.' But clearly this is not the case, as a rule, outside the laboratory. We normally make use of sensory imagery, not to satisfy either an experimenter's or our own curiosity, but to aid our adaptation to some practical situation which has arisen.

Now the image which is merely 'of a situation' appears to be most clearly connected with a general affective tendency (see pp. 322–324). Such affective tendencies, and also moods, and attitudes, are never specialised to particular and individually discriminated objects. They belong to situations as such. They are relatively persistent factors in human response, and may attach to situations which among themselves differ widely.

As we approach the primitive type of organism we find that the modes of response become relatively fewer, and presumably the number of clearly differentiated situations become fewer also. Very detailed and distinct imagery, which is bound up with some narrow and particular set of circumstances, would thus appear often to be a hindrance rather than a help. For it would bring before attention the differences between situations rather than their general likeness. It is hence plausible to suggest that primitive sensory imagery must be of a vague, schematic and undifferentiated character. As such it might well facilitate a *type* of response, while it would in no way lead to very specialised modes of behaviour.

The experiments suggest strongly that sensory imagery of this character is most frequently found to be connected with the persistence or reinstatement of some affective tendency. Whenever an affectively determined image of this kind occurred it was strikingly inadequate to effect a desired detailed reproduction. It was common at one time to speak of schematic images which are general because, owing to growth in experience, they have *lost* their particular characteristics[1]. But it has been less usual to speak of a stage in the development of image forms, in which the imagery, though it is of a sensory character, is essentially vague and schematic, because it has not yet *acquired* individual characteristics. I believe that such a stage does occur, that the function of the image at this stage is wholly to initiate and further some familiar mode of behaviour, and that its occurrence requires the persistence or reinstatement of general affective tendencies, of attitudes, or of moods, which have accompanied prior acts of perception.

It is not, of course, urged that the subjects of my experiments had actually returned for the time to a primitive state, but it *is* urged that in so far as their sensory imagery was affectively determined, they were reacting in a relatively primitive manner. And this is not surprising since the factors of affective tone, moods and attitudes, which are undoubtedly of an elemental nature, persist and may retain their old functions throughout the whole story of mental development.

(*b*) *The image as an aid to a checked reaction.*

But the sensory image in its normal occurrence, both under experimental conditions and in everyday life, has a distinctively individual character. Like the content of an act of perception, it bears all the marks of particularity. This is well explained if we take it that the direct sensory image arises in two typical situations: (1) when a set of circumstances, being only partially reinstated, the necessary practical adaptation is temporarily held in check, and (2) when response to some exciting condition is called for, but for some reason or another, ordinary forms of practical behaviour are prevented from occurring.

Supposing a reaction similar to one previously performed is checked, then a reproduction in image form of the situation in response to which it formerly occurred may arise, and may act as a reinforcing stimulus to bring about the needed reaction. This was what happened in the course of my experiments. Whenever the demand for reinstatement of some already apprehended content was checked, a definite sensory image was more likely to appear. But the principle seems to operate independently of a direct demand for reproduction. For example, as a result of very different experiments on "Conditions which arouse Mental Images in Thought," Fox maintains that his investigations "show that any delay or conflict in consciousness is a favourable condition for arousing a relevant mental image, that is one that will in some way help towards a cessation of the conflict[1]."

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[1] See *e.g.* discussion and refs. in Stout's ***Analytic Psychology***, London, 1909, II. ch. IX.

It is interesting to consider that the holding in check of an active tendency—whether to know or to do something—is precisely the most characteristic condition for the arousal of certain kinds of specific emotion, as contrasted with that merely general affective 'atmosphere' of which mention has already been made. The nature of the relation between specific emotion and sensory imagery is a matter demanding much further investigation, but nobody can doubt that the connexion is a very close one. Intense emotional discharge frequently turns an indecisive reaction into one that is both rapid and emphatic. Sensory imagery and certain of the specific emotions, therefore, may well arise under much the same conditions, and may act in concert to promote the same end.

Supposing this view to be correct, we have a perfect explanation of the confidence which, in my experiments, commonly accompanied the appearance of sensory imagery. For the primary function of the image is to manifest itself when a reaction is hesitating, and to lead to a response which is decisive. It was no accident therefore that the visualising subjects all showed superior certainty, since a sensory image, by its nature, leads to decisiveness of response. And this decisiveness is produced independently of the actual accuracy of the image.

Take next the apparent inadequacy of the sensory image in the reproduction of a correct order of sequence. This character is also perfectly consistent with the view that it is a leading function of imagery of this kind to help to produce an immediate and emphatic reaction. For the more narrow, and individual, and definite the limits of apprehension of an object or a situation, the more emphatic will the response tend to become. We often speak of the decisive man as responding "as if there were nothing else in the world but the one object of his reaction." Hence it is not to be wondered that the characteristic image reproduces as exactly as possible the sense marks of a particular situation. It is the clearness and vividness, not the order of arrangement, of the detail which are important. Order is, in fact, a general characteristic which can be represented in sensory terms only by the use of some cumbrous figure scheme, of the sort employed, for instance, by many of Galton's subjects[2].

The function of maintaining an order of sequence is, indeed, one to which nearly all forms of sensory imagery are strikingly ill-adapted while, as we have already seen, words are as strikingly well-adapted. I say "nearly all forms of sensory imagery" because it may be maintained that kinaesthetic imagery is in some respects specially qualified to perform this function. Whether this is so or not is entirely a question for experiment, and my own data throw no light whatever on the matter. But it should be pointed out that in so far as a kinaesthetic series maintains correctly an order of sequence, it never does so by virtue of isolating and responding to the relations themselves which are illustrated in the series. The kinaesthetic series is really of the nature of a habit response. If any member of the series should lose any of its characteristics, then the order of the series is also likely to be disturbed. For in such a series passage is made from one member to the next solely by virtue of some special characteristic belonging to the former. This is not necessarily the case in the vocalised series, where the relations of order may be themselves analysed, and response may be made distinctively to such relations. For then, even though in their reproduction the separate members of the series may suffer change, the order itself may—though, of course, it does not in all instances—remain undisturbed.

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[1] This *Journal*, VI. 430.

[2] See *Inquiries into the Human Faculty*: section on *Number-Forms*.

The general working hypothesis of this section demands that sensory imagery should possess such a direct clearness and vividness as would facilitate immediate and emphatic response. Now the experiments show that the clearness of the visual image is in the main a matter of 'striking' detail, and they suggest that the 'striking' character of particular detail is due to its relation to pre-existing active and affective tendencies in the individual concerned. But my visualising subjects, as contrasted with the vocalisers, reproduced in a direct and immediate fashion, and not by virtue of associations. That is, the 'striking' detail in the first case itself 'hit off' the tendency so to speak, whereas in the second case it did so often only by virtue of its association with something else. In this way, it may be suggested, the clearness and vividness of sensory imagery are direct and immediate, and belong entirely to the object or situation imaged.

The necessity for securing definiteness and individuality in the sensory image, however, comes into conflict with the equally great necessity for obtaining ready adaptation to the constantly changing demands of daily life. Thus even the sensory image undergoes constant and prolonged transformation. This constitutes at once an advantage and a danger: an advantage because, except in the psychological laboratory, occasion practically never arises for sheer, blank repetition;

and a danger because, owing largely to their incommunicable nature, those changes in sensory imagery which may lead to serious error are exceedingly likely to go uncorrected[1]. If I go into a room in the dark, thinking that there is nothing in the way, and hurt myself, the pain corrects for me my error, and I shall go warily upon another occasion. But if I merely picture a church as having a spire on it, when as a matter of fact it has none, my image cannot conflict with anything to its own detriment, and may remain incorrect. It is interesting that these experiments indicate the possibility that inventions are somewhat more likely to occur in the case of sensory imagery than with the use of words. For the introduction of new detail is what might perhaps be expected in a mode of reproduction which for the most part deals directly with individual instances.

In the relative freedom of the sensory image from correction, we have perhaps another reason for the confidence which attends its use. The image must change, since it has to serve the demands of practice in a world which never remains the same from moment to moment. But its changes induce no access of dissatisfaction since, in the main, they tend to be beyond the reach of correction.

(c) *The image as an alternative reaction.*

Imagery of the sensory type, it was suggested, is likely to occur in two typical situations. The first, that of the checked reaction, has already been considered, the second, that of the alternative reaction, remains to be discussed. In this instance response to some exciting conditions is called for, but normal adjustments are out of the question for some reason or other. The imagery which arises under these conditions has, it appears to me, a line of development entirely its own. We get here the extremely vivid sensory imagery which is characteristic of dreams, of reverie, of fatigue, of certain states of drug intoxication, and the like. This is the type, and this is the general condition, of a considerable amount of the imagery which has a large part to play in the development of the arts, and its consideration is fundamental to the construction of a sound psychological aesthetics. The majority of the factors which determine the form, subject-matter, and clearness of such images operates in the manner which Rivers has called 'unwitting[2].' But the experiments which I have described in the early part of this paper do not deal with the image as an alternative reaction, or with its peculiar problems. Consequently I do not now propose to attempt to discuss the questions involved.

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[1] Cf. J. Philippe: "Sur les Transformations de nos Images Mentales," *Rev. philos.* 1897, 481–493.

[2] *Instinct and the Unconscious.* Cambridge, 1920, 16.

(*d*) *Why words tend to be substituted for sensory imagery.*

As we have seen, the marked individual character of sensory images, and the attitude of certainty which attends their use, arise in the main from the function of such images as aiding a checked reaction. But the whole trend of mental development is away from the particular and the individual. Analysis and construction come to play a greater and greater part, and by their aid, situations, which, as regards many of their characters, differ very widely, are brought into relation. No method of reproduction which is largely confined to the reinstatement of individual instances can possibly remain adequate if intellectual and practical development is to proceed. In particular it has often been pointed out that the apprehension of general relation of likeness, which may lead to all kinds of arguments from analogy, is of the greatest importance for intellectual advance.

Now the data of the experiments suggest as strongly as possible that it is precisely the analysing and relating attitude that the use of words favours. As compared with the visualiser, who goes directly to his particular material, the vocaliser commonly secures his reproduction of an absent object or situation by an indirect method. He relies upon association. Moreover the associations are in the majority of instances suggested, not by the object considered as a whole, but by some feature of the object. Indeed many divergent associations are often formed, some with this detail and some with that.

Here we find one of the reasons why vocalisation as compared with visualisation proved to be accompanied by an attitude of uncertainty. The associations formed in connexion with a single object frequently varied so greatly, that when, at the period of reproduction, a tendency arose now to accept and employ this association, and now to revive and employ that one, hesitation was bound to supervene. For according to which association was used the actual reproduction would vary in its details.

Again very probably the decrease of confidence is due to the fact that, on the whole, the employment of words in reproduction leads away from the reinstatement of material in close relation to emotions. Why and how this should be the case are matters of extreme interest, but are beyond the limits of the present paper. It is worth noting, however, that while development along the line of the sensory image proper may lead, as has already been suggested, to the arts, development along the line of reproduction through the medium of language leads to thinking in the strict sense, with its abstract character and its relative freedom from emotional disturbance.

A third reason for the doubt which often attends vocalisation methods may be due to the fact that words are our habitual instruments of discussion, while obviously no person can conduct a dispute in terms of sensory images merely. A false statement can be as readily checked as, under certain conditions, can a false overt movement. And this consideration should make it clear that the lack of confidence consequent upon the use of words is no serious drawback, for it may easily be dispelled. It does not need to await the sort of Jack-in-the-box, haphazard, appearance of some new sensory image, but can be subjected to the confirmation or denial of a second observer.

The very fact that, by the aid of words, objects and situations are retained and reproduced the more clearly according as they are brought into relation with others, suggests a further great advantage of vocalisation. In these experiments while sensory imagery broke down badly in its attempt to deal with an order of serial relation, the use of words dealt adequately with such an order. Not only do words facilitate an attitude of analysis, but they provide instruments by which general relations may be analysed, and may be indicated fully as definitely as particular objects. In fact if there is any characteristic error incident to the use of words in reproduction it is that of blending or confusion. And this is traceable to that very function which sensory imagery is barely able to perform, the formulation and response to rules and laws by which individuals are related.

Yet with these advantages words lose nothing of the necessary fluidity characteristic of sensory imagery. The capacity for transformation is clearly essential, and, so long as it can readily be checked, wholly advantageous; for all modes of reproduction are primarily meant to serve the purposes of practical adjustment.

It thus appears that words are able to maintain the advantages of sensory imagery, while in addition their ease of communication, and the greater range of situation with which they can adequately deal, make them capable of rendering further development more ready and certain.

An interesting chapter in genetic psychology yet remains to be written on how words actually come to supplement, and, in some cases, to replace, direct sensory imagery, and how, at the same time, the sensory image is not only normally retained as an element in mental life, but may develop along lines of its own, which lead to types of human response of the very greatest psychological interest.

In this paper I have, for the purposes of clearness, spoken sometimes as if sensory imagery and methods of vocalisation are separate, and even, to some extent, antagonistic. But in actual life, as need hardly be said, they are most frequently mingled. The nature and conditions of the precise help that each can give to the other must be left over for future discussion.

(*Manuscript received* 27 *August*, 1920.)

A CASE OF SOMNAMBULISM.

By S. M. HAGGARD.

In 1916 I was teaching at a girls' school and chanced to set some of the elder girls, aged about 16½ years, the following essay: How far were Shakespeare's Sonnets characteristic of the Poetry of the Elizabethan Age, and how far did they differ from it?

One of the girls, who had been vainly trying to think of points for her essay, took up to her bedroom a pencil and paper in case she should wake early the next morning. She left them on her dressing table and on washing in the morning was astonished to find them on a table by her bedside. She then noticed that the paper was covered with pencil notes, in her own handwriting (see accompanying plate), which she had no recollection of thinking out or of writing.

She brought the notes to me to ask if she might make use of them for her essay in view of the circumstances in which they had been written. She thought that it would have been unfair to have done so without asking permission. At the same time she pointed out that after having seen what she had written during her sleep, it would be quite impossible for her to compose the essay on any other lines.

Permission was given to make use of the notes, and the girl's essay was the best sent in, considerably above the usual level of her work at that time. I can vouch for the fact that the ideas contained in the notes were in no way suggested by my own class teaching, nor had the girl had access to any books bearing on the subject.

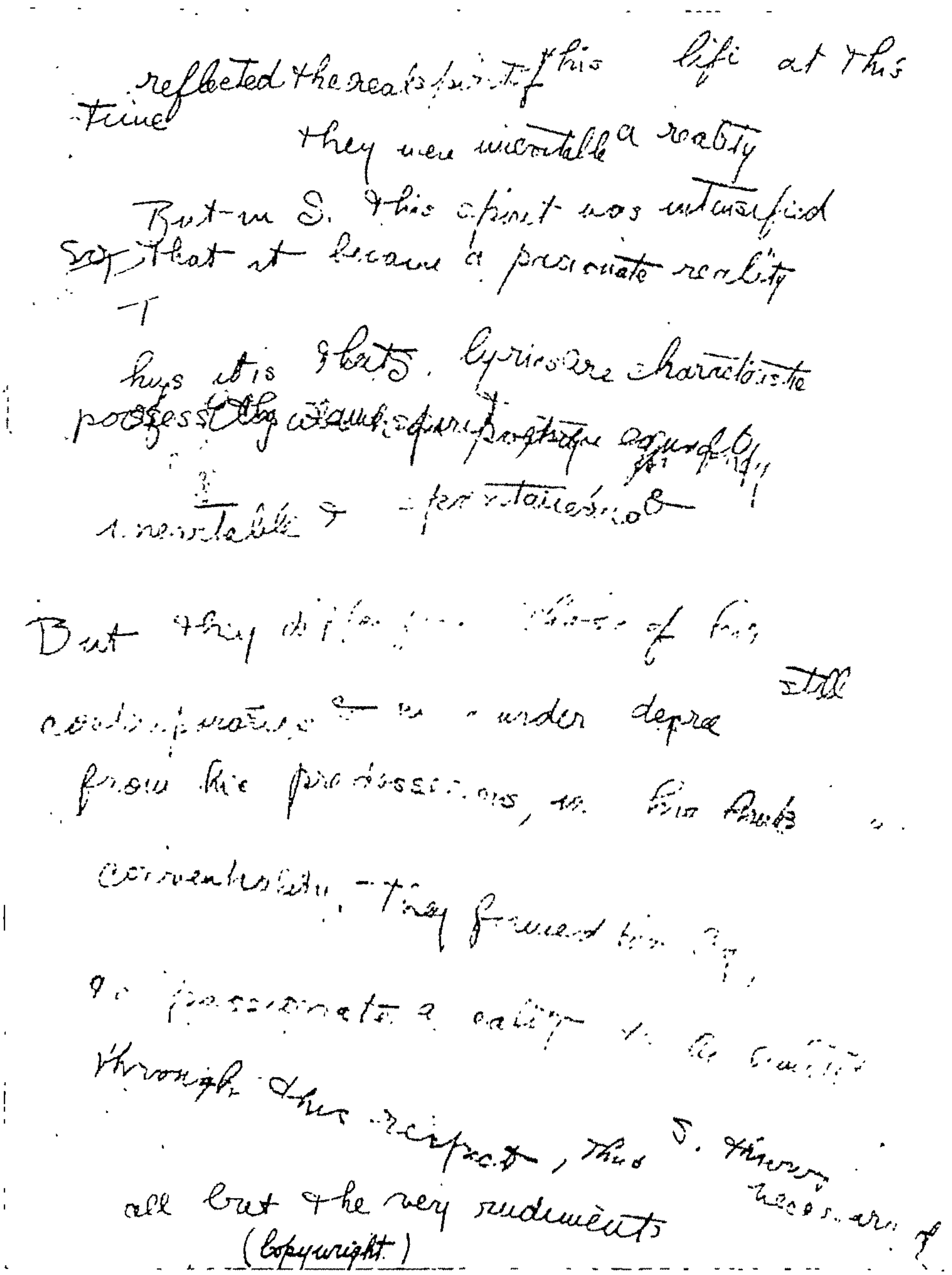
The following is the transcript of the notes written. The original is reproduced in Plate III.

...reflected the real spirit of this life at this time, they were inevitable, a reality.

But in S.[hakespeare] this spirit was intensified so that it became a pasionate reality.

Thus it is that S.[hakespeare] lyrics are characteristic of the Elizabethan poetry for they possess the same spirit, were equally inevitable and spontaneous. But they differ from those of his contemporaries and in a wider degree from his predessessors in his lack of conventiality. They formed too big, too passionate a reality to be limited through this respect. Thus S.[hakespeare] throws all but the very rudiments... necessary of....

PLATE III



time reflected the real spirit of his life at this
they were inevitable a reality
But in S. this spirit was intensified
so that it became a passionate reality
Thus it is that S. lyrics are characteristic
possess [illegible]
inevitable & spontaneous

But they differ from those of his
contemporaries — [illegible] under depra still
from the professions, in their [illegible]
conventionality. They formed [illegible]
a passionate reality to the [illegible]
through their respect, thus S. throws
all but the very rudiments necessary of
(Copyright)

It will be observed that the girl has jestingly written the word 'copyright' (mis-spelt) at the bottom of the sheet of notes. This word she wrote before finally parting with the paper and serves as the only available means of comparing her normal handwriting with the handwriting of the notes, as the event occurred over four years ago, and the girl has long ago left school. The following sentences, however, are of psychological interest, extracted from letters written by her to the Editor of this *Journal*: "I am afraid I had forgotten the incident of this essay or that any notes were in existence, until I received Miss Haggard's letter a few days ago." "I cannot remember what the notes were or what they looked like, and though I have enquired about the subject of the essay, the title of the subject conveys nothing to me." "Now that I have seen the notes, I certainly seem to recognize them as having been written by me, that is to say, I seem to recognize the idea running through them... only I cannot understand how I should forget having written them." "You ask if I have ever been known to do any sleep-walking. As far as I know I have done so twice. Once while I was ill and once later for no apparent reason. [Then] I did not leave the room or do anything else than walk up and down the bedroom. I have a brother who sometimes talks in his sleep, but no other members of the family have been known to do anything."

CRITICAL NOTICE.

BY F. C. BARTLETT.

The Origin of Man and of his Superstitions, by CARVETH READ, M.A., Lecturer on Comparative Psychology (formerly Grote Professor of Philosophy) in University College, London. Cambridge: The University Press, 1920. pp. xii + 350. 18*s*. net.

Poets, priests and philosophers have always sung, dogmatized and speculated about the origin of man. Since the birth of scientific method they have been joined in their quest by the most varied company of investigators. Biologists, physiologists, neurologists, anatomists, zoologists, geologists, anthropologists of every kind and degree have all passed beyond their more immediate concerns to suggest the modes by which man may have lifted himself to his present place in the world. "The quest of origins," says Prof. Carveth Read, "is fascinating"; and, of the many statements made in this book, here is one which is perhaps least of all likely to arouse controversy.

It is as a psychologist that Prof. Read approaches his problem. He loses no time in stating his hypothesis, and no opportunity of repeating and enforcing it. The very first section of the book is entitled "The hypothesis," and the position on which the whole of the work is said to be based is stated clearly in the second paragraph of this section:—the chief differences "between man and his nearest relatives" may "all be traced to the influence of one variation operating amongst the original anthropoid conditions. That variation was the adoption of a flesh-diet and the habits of a hunter in order to obtain it." Further a flesh-diet apart from hunting "could have done nothing for the evolution of our stock." The adoption of the hunting life, therefore, is the essential variation upon which everything else depends. Nothing, the author is assured, could be more certain than that some members, or member of the anthropoid stock adopted a hunting habit, and that from this follows the whole long story of human development.

In the first two chapters a variety of mental and physical characteristics of man are traced more or less immediately to the toils, pleasures or duties of the chase. Man's life on the ground; his customary erect attitude; the specialisation of legs and feet and particularly of arms and hands; reduction in the size of jaws and teeth; the development of a big brain; the adoption of social life; the birth of language proper; the production and use of fire; a slight shortening of the alimentary canal; the loss of seasonal marriage; the acquisition of a relatively naked skin; the cannibalism prevalent at certain stages; the extreme variability of racial characteristics; disposition to slay and destructiveness; perceptive, contagious and effective sympathy; hostility to all who are outside the hunting group; the institution of property; leadership; subserviency; emulation; social orders of precedence; intelligent coöperative behaviour; the constructive impulse; monogamy; war and all of its results; play; social eating and weeping; the gradual moralisation of man—all these are traced directly or indirectly to hunting practices. The development of the higher mental functions can be traced to the same source also: "The necessity of learning the whole art of hunting from its rudiments," he writes, "without the help of instinct or tradition, by sheer observation, memory and inference, put extraordinary stress upon the brain. At first by knowledge, strategy, coöperation and persistence of will, later by devising weapons and snares, evolving language and discovering the ways of making and utilizing fire, man found means of entirely changing the condition of his life." Thus Eoanthropus needed, and developed, a big brain, and with this "a continually increasing fecundity of ideas." The life of the imagination grew directly out of the manifold activities of the hunter, and once it was rendered possible, assumed most important functions in strengthening and reinforcing the social bonds which were first forged by the chase. This it did by virtue of its intimate relationship to belief. So the book passes from a consideration of the origins of man to a study of the growth of superstition.

At this point the volume takes a great leap forward in interest and importance. The first two chapters contain some facts, no small amount of most ingenious speculation, and much excellent writing. But many readers will feel that hunting is made to bear an over-large burden. There seems little reason to suppose that the primitive hunter worked entirely without the aid of instinct or tradition, or that hunting is more than one of the many conditions of human development. In fact the author himself indicates (p. 48) that other kinds of associations, besides that of the hunting pack, might well favour the development of certain of the characteristics of man. Why should it be necessary to trace all types of human response back to one common motive?

Chapter III consists of a most admirable study of Imagination, its relation to Belief, the main causes and grounds of Belief at different levels of mental development, and the influence upon primitive modes of behaviour of Reasoning and Ideas. The study of 'play-belief' is of particular interest, though it may be doubted strongly whether the savage has 'no seniors,' and only 'tardy experience or social fatigue to check' the vagaries of his imagination. It was also well worth while to insist upon the part which "general ideas at the savage level" may play, and there are a number of good illustrations of this part in the subsequent discussions of Magic and Animism.

Chapters IV–VII deal with Magic, Animism, the Relations between Magic and Animism, and Omens. Here are gathered together numerous illustrations of the operation of the processes discussed in the third chapter. Magic is defined as a connexion of events imagined as constant, and dependent upon some impersonal agency. This is something like Frazer's idea of Magic as a sort of primitive natural science, but the author dissents definitely from Frazer's views as to its relation to Animism. Magic comes in when the cohesion of the hunting pack has weakened; its main social purpose is to strengthen the authority of the elders. Animism is either 'hyperphysical' when it is an imagination-belief that various natural occurrences are due to the direct action of some kind of spirits; or 'psychological,' when it marks the attitude of regarding all things, animate or inanimate, as having a certain consciousness, and all reactions as voluntary. On the whole the author inclines to the view that Magic is prior to Animism, though commonly the two are found closely intermixed. "At any rate, their origins are independent": their subsequent relations may be varied.

Leaving without comment the three extremely interesting chapters on Omens, The Mind of the Wizard, and Totemism, we come to the final study on Magic and Science. "Magic and Science have, indeed, some common ground; for both are products of our poor human mind, which is sorely pestered in explaining its experiences by the notion of 'forces' that somehow bring about events, and which cannot get on at all without assuming uniformities of nature." But, each starting from Common Sense, Magic soon distorts its facts, while Science systematizes them. The differences which arise are due, in fact, to psychological considerations. For the determining beliefs of magic are "incoherent and vague," those of science are "coherent and definite." Though the procedure of Magic and that of Science differ, yet the magician is certainly the precursor of the scientist. "In the course of thousands of years some men grew more interested in the positive than in the magical side of their profession, and became scientists."

In this discussion it may be that the author allows hardly sufficient importance to the tendency that may occur, or develop, to seek direct relations of likeness among apparently divergent facts. Perhaps one of the important determining conditions of magic is the attempt to guard certain groups of observations from outside disturbance. It seems possible that sometimes the scientific imagination is incoherent and vague, and the magical 'coherent and definite.' But the latter may be excessively limited. Earlier, in fact, Prof. Read says, "The natural progress of reason consists in relegating analogies to poetry and rhetoric" (p. 98). It may be doubted whether a study of the history of scientific advance would justify this statement. He repeats the suggestion in the final chapter: "for scientific progress it was necessary that reasoning by analogy should be abandoned." As a matter of fact, however, abandoned it never really has been, and science has grown nevertheless.

The book closes upon a gloomy note. Many of the most noble and desirable of the qualities of man, particularly as these have been manifested in social life, have, it is urged, been both acquired and maintained largely "under the influence of animistic illusions." These illusions are fast being dissipated, and with them may go many of the virtues of loyalty, solidarity, and social sympathy upon which human well-being is based. Further, from our beginnings in the hunting pack we have relied to the greatest extent upon leaders. Thus individuality has been but slightly developed, and the majority of mankind are of low average capacity. We grasp and let slip imagination-beliefs in the most careless manner, and irrespective wholly of their worth. But after all, is the conclusion, it is not much good to guess: "The only safe reflexion is that he who lives longest will see most."

Thus closes a thoroughly interesting book. The volume does not stand or fall according to the fate of its 'hunting' hypothesis. Altogether apart from that, it possesses the greatest importance. It raises a very large number of fascinating questions, and discusses them all with great skill and decision. It represents a vast amount of careful research, and is as pleasant to read as it is profitable. The psychological portions of the discussion are the best; and that is to say a very great deal, for it is here chiefly that the majority of the books which have dealt with similar subjects have been most sadly wanting.

CRITICAL NOTICE.

By F. C. BARTLETT.

The Group Mind. A Sketch of the Principles of Collective Psychology with Some Attempt to Apply them to the Interpretation of National Life and Character. By William McDougall, F.R.S. Cambridge: The University Press. 1920. Pp. xvi + 304. 21*s.* net.

It is safe to say that no book on psychology published in this country has been more widely read or more keenly discussed than Prof. McDougall's *Introduction to Social Psychology.* But that volume was proposed merely as a first rough sketch of the subject matter, and principles of a vast field of study. A very large number of readers have thus awaited with interest the promised sequel. To some extent many of them have meanwhile withheld criticism. Though they may have been convinced, for example, that McDougall's treatment of instincts, interesting and suggestive as it is, is incomplete and schematic, they have restrained their impatience, waiting for the fuller discussion that was promised. Now the time of their freedom has arrived, and it is greatly to be hoped that they will take advantage of it.

The Group Mind, its author says, "builds upon" *An Introduction to Social Psychology.* This at once suggests that he is perfectly satisfied with the foundation laid twelve years ago. So much is this the case that the present book, in spite of the great interest and insight of many of its passages, must strike most readers as adding practically nothing of a fundamental nature. Indeed, the author himself recognises this: "Although my fellow-travellers have not entirely approved my outfit, I have launched out to put it to the test"; and "I cannot hope that my readers will follow me if they have not at their command a similar outfit—namely a similar view of the constitution of human nature." Perhaps the best review of *The Group Mind* would consist of a thorough criticism of *An Introduction to Social Psychology.*

However, to describe this book by itself: it is divided into three parts, the first concerned with "General Principles of Collective Psychology"; the second with "The National Mind and Character"; and the third with "The Development of National Mind and Character."

To the whole is attached an Introduction in which McDougall deals with the "province of collective psychology." He there makes his position clear: that social psychology has to do with the reactions of groups as such, and this at once plunges him into the extremely ancient and arid discussion as to whether there is a 'group mind.' Since 'mind' is to McDougall "a system of purposive forces," and it is clear that any group must illustrate a more or less perfect organization of individual tendencies each of which can be regarded as possessing some direction, or goal, it follows that he finds it easy to talk of 'group mind.' And just as in the *Social Psychology*, he treated individual human behaviour as the result of a conflict, or an integration, of a few root tendencies, so in the present book he considers group reactions as the result of a conflict, or an integration, of the tendencies of the individuals who make up the group. But there is an important difference between the two cases. In his earlier book McDougall did, at least, attempt an original study and analysis of human conduct. In the present book he starts with all his root tendencies laid out before him. All the time one feels that this is hampering to any real, unbiassed, analysis.

The first Part describes general characteristics of different types of group organization, as exemplified in the loosely organized crowd, and the strictly organized army. The second Part concentrates upon the "most complex and important kind of group mind, namely the mind of a nation-state." Many most interesting topics are here discussed: what constitutes the mind of a nation; how far a nation-state demands mobility in every sense on the part of its members; the rôle played by leaders in national life; what can be meant by a national will; how it comes that ideas are said to exercise a determining influence upon national destiny; and what characterizes the 'higher' type of nation. Part Three discusses, with a considerable wealth of historical illustration, how nations have grown up; how, when they have already become established, they may yet suffer important change; and finally how their development in the period of their most rapid growth can be differentiated from the character of the advance made once they have won their way to maturity.

In the very first chapter of the book, McDougall reflects on the oft-noted opinion that while "participation in group life degrades the individual," yet "only by participation in group life does man become fully man." "The resolution of this paradox," says he, "is the essential theme of this book," which shows how group organization "counteracts degrading tendencies; and how the better kinds of organisation render group life the great ennobling influence by aid of which alone man rises a little above the animals and may even aspire to fellowship with the angels." The reader will wonder whether he is going to be concerned with psychology or with ethics. As a matter of fact, the two tend to jostle one another all the way through. No doubt it was under the influence of ethical enthusiasm that McDougall turned aside to frequent vituperation of the German, and to repeated laudation of the American and French races. The attitude was natural, and perhaps admirable, during the war. It is a blot upon a book intended to be permanent, and helps to give to many of its pages a character of merely occasional interest, as if one might be reading in a newspaper.

Nevertheless every chapter throughout the book, and particularly in Parts II and III, provides material which might well form a basis for prolonged discussion. For if McDougall introduces no strikingly new principles, he certainly attempts to apply the principles he has elsewhere formulated to the most important problems in the character and development of nations. Take, for example, his treatment of the place of ideas in national life. This follows immediately upon a discussion of the 'will of a nation.' Consequently it begins by a reiteration of the part that ideas may play in volition. Just as in the case of individual acts of will, consideration of a situation in relation to the whole self is involved, so in the case of the 'national will,' the situation desired must be considered in reference to the idea of the nation as a whole. This 'idea of the nation,' however, is by itself ineffective, and as effectiveness is contributed to the idea of the self by the self-regarding sentiment, so the nation-idea is able to exert actual influence upon practice by the development of the nation-regarding sentiment. "The idea alone, as an intellectual apprehension, cannot exert any large influence;...it determines judgment and action only in virtue of the sentiment that grows up around" its object (p. 170).

McDougall gives an illustration as, in fact, he is always ready to do:—"Imagine a people in whom anti-nationalism (in the form of cosmopolitanism, syndicalism, or philosophic anarchism) had spread, until this attitude towards the nation-state as such had become adopted by half its members, while the other half remained patriotic. Then there would be acute conflict and discussion and the idea of the nation would be vividly present to all minds; but the nature of the sentiment attached to it would be different, and opposite in the two halves; one of attachment and devotion in the one half; of dislike, or at least indifference (*i.e.* lack of sentiment) in the other half."

Surely it is an incomplete analysis to present this as a case in which different sentiments are attached to the same idea. A cosmopolitan usually has his own idea of a nation-state, and this idea differs from that of the nationalist. There is in general, nowadays, far too marked a tendency to assume that because ideas do not operate *apart* from sentiments, or from some definite affective accompaniment, ideas really contribute nothing *in conjunction with* sentiments, or whatever helps to bring them into play. For all his emphasis upon the importance of the development of self consciousness, and group consciousness; for all his insistence upon progress through 'emancipation of the intellect' (see particularly pp. 297–301 of the present book)—McDougall appears at times to share this tendency. We require a new, thoroughgoing and unbiassed analysis of the part that ideas as such may contribute to a determination of the directions and ends of conduct. Particularly is this necessary in social psychology. A first-hand study of modern labour movements, for example, carried out from a strictly psychological standpoint, would probably throw new light upon the effectiveness of ideas in helping to shape social organization and institutions. It is not enough to assert that this idea, or that one, has been important in social development. We must know how, if at all, an idea, once it has been formed, helps to determine its own emotional colouring. We must be prepared to show how, if at all, the idea, as an idea, operates, and what varied effects different types of idea may produce. And discussion of these matters, if it is to be convincing, must not be shy of passing into details.

Any idea that can be truly called national must, as McDougall most rightly urges, have reference to 'the whole,' and not merely to 'all the members of' a nation. But his argument as it stands in this chapter (Pt. II, Ch. XII) is hardly conclusive. A nation has an indefinite future, it is urged, and hence 'all' at any moment, never really includes 'all' of the group.

Now suppose we should consider, instead of the nation, a short-lived group, formed with a limited membership, for a prescribed period. In this case it might be possible to consider the needs of literally 'all' the members of a group. Yet many would still maintain, and probably McDougall among the others, that the 'good of all' would not be identical with the 'good of the whole.' In social grouping the whole is "much more than the sum of the existing units," not simply "because it has an indefinitely long future before it, and a part to play through indefinitely long periods of time" (p. 171), but because social coöperation does actually bring new psychological factors into being, which, when we are considering individuals alone, do not come before observation. However, these remarks point to a deficiency in this particular passage only, for McDougall himself, in other places, urges as strongly as possible the uniqueness of many of the factors produced through social grouping.

The consideration of "ideas in national life" concludes by a study of "The Value of Nationality." No small part of this most interesting section has a distinctly ethical trend, yet there is much important psychological material in the discussion of how the ideas of "liberty, equality, progress and human solidarity" have helped to shape social organization. Still the precise manner in which ideas operate is left in need of some explication. In this part of the book the terms 'idea' and 'ideal' are used, without comment, almost as if they were interchangeable terms. A careful psychological analysis of their nature and their relations is most desirable.

The study of the development of nations, to which Part III of the volume is devoted, is throughout deeply interesting. The discussion of racial changes during the historical period—which most explicitly denies any "considerable changes of innate qualities"—and the comparison of the nature of the progress of nations in youth and in maturity, raise an enormous number of questions of the first importance. And upon all these questions McDougall is, by a very admirable quality, ready with a decisive answer. The last two chapters are particularly stimulating. McDougall has no doubts as to the character and basis of human progress, past, present and to come; and his views are expressed with a vigour and swing that no reader can fail to enjoy.

Nobody can deny that *The Group Mind* is worth reading. It has many interesting things to say; it provides much food for thought; in many parts it is delightfully written; and it will doubtless form the starting point for many a vigorous debate. Yet it is undoubtedly a disappointment. Twelve years ago its author constructed a scheme, and into it he fitted many of the facts of human behaviour. It was a scheme boldly and brilliantly conceived, but in the opinion of many it was too simple to meet the demands laid upon it. Now with the same scheme in mind—nothing added, and nothing retracted—he strives to set in order the enormously greater complexity of group conduct. In the result it is the scheme that must suffer.

CRITICAL NOTICE.

BY T. H. PEAR.

Instinct and the Unconscious, by W. H. R. RIVERS, M.D., D.Sc., LL.D., F.R.S. Cambridge: The University Press. 1920. pp. viii + 252. 16s. net.

The general aim of this book is "to put into a biological setting the system of psychotherapy which came to be generally adopted in Great Britain in the treatment of the psychoneuroses of war." And, since the exposition is consistently psychological, one may profitably inquire before reading it what conception its author holds of the scope of psychology; an important question in these days. It is given on page 43: "the special task of psychology" is "to understand the place of consciousness in relation to behaviour."

"The main function of a psychoneurosis," Dr Rivers states, "is the solution of a conflict between opposed and incompatible principles of mental activity." In learning the nature and evolution of those principles and the manner in which their conflict is solved the reader will be introduced to an argumentative texture of extraordinarily close weave, containing scarcely a stitch too many; exhibiting a striking and, in many ways, a novel design. Whether, on reaching the end of the book, the student of psychology will find that this new design can be harmonized with the rest of his thought-patterns will be determined by his opinion concerning the closeness with which this garment of theory fits the facts. Dr Rivers himself clearly points out loosenesses here and there.

One demand upon the reader is made which renders any rapid progress through the book impossible. From the very beginning he will be required to disabuse his mind of most of the meanings which it has attached painfully, and with many setbacks, to the current terms of psycho-analysis; even of psychology itself. The Unconscious, Repression, Dissociation, the Complex, Instinct, by the time the end of the book has been reached all these have been 'taken down' and re-built. But the author compensates to some extent for this by freely allowing the reader to see him at this task of dissection, and by explaining, step by step, why such re-moulding seems to him to be necessary.

This re-making process begins, not unexpectedly, with the Unconscious. Three examples are given to illustrate the author's conception of 'unconscious experience,' the distinguishing mark of which is that it "is not capable of being brought into the field of consciousness by any of the ordinary processes of thought or association, but can only be recalled under certain special conditions, such as sleep, hypnotism, the method of free association, and certain pathological states." And "if an idea springs spontaneously into the mind without obvious antecedents in consciousness" Dr Rivers does not speak of it as unconscious but as 'unwitting.'

Repression, with its relation to the process which in this book is called Suppression, is discussed at length, for a grasp of the difference between them is vitally necessary to any understanding of the main theory. And here is seen the use of the term 'witting,' introduced in the preceding chapter. The process by which experience becomes unconscious is called 'suppression,' but 'repression' is used for the process by which we wittingly endeavour to banish experience from consciousness. Repression, moreover, is only one of the ways in which suppression occurs; a conception which is helpful in answering the common objection to the theory of active forgetting: why, when we try to forget a thing, do we usually fail?

In this treatment of forgetting, of which suppression is one form, the great debt owed to Freud for emphasizing the rôle of repression is fully acknowledged. In fact, the treatment of forgetting follows orthodox Freudian lines fairly closely, and it is not until the next chapter, "Suppression and Inhibition," that the book leaves the beaten track. But now its deviation is at right angles to the path, for it plunges into physiology. Its entry is made at that point in time when, in 1902, a few snicks in Dr Henry Head's arm changed the dull text-book chapters on cutaneous sensation into material for the most fascinating speculations upon the behaviour of our own remote ancestors, and, incidentally, gave science two words whose utility is matched only by their euphony: 'protopathic' and 'epicritic.' It is these terms, and the meaning which has grown up around them, which are pivotal to much of Dr Rivers's argument. He claims that when, after operation, epicritic cutaneous sensibility returns, the earlier protopathic sensibility does not persist unaltered side by side with the later development, but that certain of its characters fuse or blend with the epicritic, while others are not only suppressed, but—this is the important point—they persist in a latent form, ready to come again into consciousness if the appropriate conditions are present. That is to say, Freud's conception of the unconscious finds its parallels even at the sensory level.

Dr Rivers then passes to the work of Head and Gordon Holmes, who discovered, between 1911 and 1912, a relation between the cerebral cortex and the optic thalamus very similar to that existing between protopathic and epicritic sensibility. When the cortex cerebri is put out of action, by injury or otherwise, stimulation of the skin produces sensations overweighted affectively, very similar to the experiences of protopathic sensibility, while there is in them an absence of objective character very similar to that observed in this form of sensibility.

He believes that under ordinary conditions the cortex suppresses the affective over-response of the thalamus.

It is, however, the recent work of Head and Riddoch which suggests the most impressive confirmation of the theory of suppression. In 1918, their observations of a number of patients in whom the spinal cord had been completely divided enabled them to examine the functions of the lower end of the cord when isolated from the rest of the nervous system. The diffused and generalised 'mass-reflex' which they discovered (a reflex possessing characters quite unknown when the nervous system is intact,—with its generalised character, its absence of discrimination and localisation, its strong suggestion of a mechanism adapted originally to remove the whole rather than a part of the animal from noxious stimulation—) is considered by Dr Rivers to have suffered, in the normal human being, complete suppression in favour of reflexes delicately graduated according to the locality and, to some extent, according to the nature of the stimulus. This method of reaction he believes to have lain hidden in man, as a potentiality which is scarcely ever realised; as a living embodiment of an earlier phase in history. In the history of *the* man or of Man? Dr Rivers inclines to the latter view.

> In this case, and in the case of protopathic sensibility, we are not dealing with the suppression of individual experience, but with the suppression in the race of experience belonging to the earlier phases of its history. Through a special experimental procedure, or through the accidents of war, it has been possible to follow the suppression of this experience in the individual. The fact that this is possible suggests that the racial suppression is repeated in every individual as part of the recapitulation of the racial history. If this be so, however, the suppression takes place at so early an age that its detection is impossible. It would never have been suspected if the experiment and the clinical observations of Head had not pointed the way thereto.

Suppression, then, is exhibited as a process which may take place on the sensori-motor, the reflex or the highest levels, and the 'becoming-unconscious' of experience is merely a special variety of the process of inhibition, common to every phase of animal activity. But the relation of lower to higher performance in the nervous system is carried still farther. Just as on the sensori-motor level certain elements of protopathic experience are utilised by the process of fusion while others are suppressed, so, it is suggested, two kinds of elements in the unconscious may be distinguished:

> Corresponding with this distinction two kinds of elements in the unconscious might be recognised—those which have only disappeared from consciousness in their original form, but continue to exist in the different form they have assumed through the process of fusion, and those whose disappearance has been more complete so that they do not enter into consciousness even in an altered form under normal conditions, though their continued existence is shown by their reappearance under peculiar conditions such as those which accompany the regeneration of a divided and reunited nerve.

To the second class only, to the dwellers in the mind's Siberia, does Dr Rivers propose to give the name unconscious.

Here indeed is food for thought, but the food is so novel that one scarcely knows how to attack it. Let us begin with what is, to our taste, its most attractive part.

The British Psychological Society's symposium, in 1914, on *The rôle of repression in forgetting*[1] left in the mind of the present reviewer two vivid impressions. The first was that no scientific progress would be made in defining further the concepts underlying Freud's incalculably valuable practical hypothesis of repression until it ceased to be regarded merely as something morbid, reprehensible, unwise, shortsighted, and began to be approached from the positive standpoint, the standpoint of psychology, in short. The other was a growing appreciation of the futility of attempting to explain forgetting by invoking pseudo-physiological conceptions which were neither invented nor sanctioned by physiologists. When we understand the mechanism of a 'neurogram' even half as well as we now understand that of a telegram or a phonogram it may be time to lean upon such a conception, but not before.

Now Dr Rivers clearly presents suppression and repression as processes which are necessary to the efficient working of the mind. In certain circumstances they may lead to unfortunate results for any particular individual, but this fact does not constitute an insoluble biological problem, rather the reverse; for breakdowns often reveal more clearly the nature of a mechanism. To them he adds the conception of fusion, of elements integrated into men's present experience, perhaps of

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[1] This *Journal*, 1914, VII. 139–165.

> ...stepping stones
> Of their dead selves...,

but, by their harmonious combination, distinguished from those elements which, being neither stepping-stones nor dead, appropriately enough form the most prominent material for the psychological discussions of this troublous twentieth century. There is, however, one question which still remains unanswered: what kinds of experience become fused? If we suppose that the compatible, the dull, the inoffensive in our past subside into a compact, undifferentiated mass; that the numbers of our tram-car tickets, or of our hotel rooms, the details of our everyday meals, of our casual conversation, fuse into a drab solid background, and are thenceforth inextricable as individual experiences; how do we account for the surprising way in which we occasionally encounter them years later in dreams, or for the hypnotist's power of dragging up and saving from oblivion the minute details of a recent experience?

Dr Rivers may reply, and probably justifiably, that this is evidence that such recallable material was *not* fused, and that for every item so dredged up there are thousands which lie for ever at the bottom; that the fact that any part of our past appears in a dream is strongly suggestive that it is not of a fusible character. But it all takes us farther and farther away from the calm pages of Ebbinghaus, and inclines us regretfully to search for some other name for the 'curve of forgetting.' Yet it must not be forgotten that these illuminating suggestions are themselves based upon analogies with physiological findings. And those findings themselves must, of course, be distinguished in thought from the interpretation placed upon them by physiologists. To discuss the validity of these latter interpretations is beyond the powers of the present reviewer. But the particular part of Dr Rivers's speculations which seems to require most careful thought—all the more careful because of its inherent fascination—is the suggestion that the result of any injury to a formerly intact nervous system is or represents a past phase in the development—phylogenetic, ontogenetic or both—of that system. Dr C. S. Myers has pointed out elsewhere[1] that if owing to some nervous lesion a man develops a new gait, we cannot assume that this gait represents a more primitive form of progression. So, it may be argued, the appearance of an unusual mental state when a breakdown of higher control occurs cannot be regarded as proof that this state represents a primitive phase in man's ancestry.

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[1] *Discovery*, November, 1920, 339.

A view of Instinct is now proposed which is compatible with the beliefs already set forth. An attempt is made to distinguish instinctive from intelligent behaviour by "something in the nature of the behaviour itself." The instincts are divided into two classes. The first, or protopathic, are distinguished by "the absence of exactness and discrimination of appreciation, and of graduation of response," "the character of reacting to conditions with all the energy available," and "the immediate and uncontrolled character of the response"; the second, or epicritic, by their freedom from this 'all-or-none' principle and their capacity for graduation of response. It is impossible here to give any account of the evidence adduced for this division, and the reader will be reminded of a recent discussion of this part of the theory[1].

A central feature of the succeeding chapters of the book is now introduced in the discussion of what are termed the 'danger-instincts.' Five chief forms of reaction to danger are distinguished; flight, aggression, manipulative activity, immobility and collapse with trembling. An attractive explanation of the last-named biological puzzle is offered; in it is the penalty which Man has to pay for the pliancy of his danger-instincts, for their failure to become systematized or fixed in any one direction.

It is the place assigned to the undoubted and undoubtedly important reaction of manipulative activity which, however, seems difficult to understand. That it is classed, and not inadvertently, with the instinctive reactions seems clear (see p. 56, "these five main forms of instinctive reaction to danger"). Yet the paramount fact about this happy method of meeting danger seems to be that, in civilised man at least, it is not instinctive, but in most cases involves the possession of some highly artificial skill; often, too, of some intricate machine through which such skill may operate. Dr Rivers himself cites two ultra-civilised examples, those of driving a motor car and piloting an aeroplane. And this raises the important question whether in such very un-instinctive reactions the undoubted fact that there is often no affect can be taken as evidence that the affect has been suppressed. Is it not probable that in many cases there is no realisation, conscious or unconscious, of the danger? The performance in traffic of the average youthful rider of a motor-cycle does not strike one as an example of suppressed affect, though it may become one if he has recently had a bad smash. To point out the importance of manipulative activity in the presence of danger is timely and significant, but we believe that it has crept into the wrong part of this book.

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[1] This *Journal*, 1919, x. 1–42.

One puzzling phenomenon of human behaviour—'the faint'—might profitably have been considered in this connexion, for in many ways it seems to be relevant here. Is fainting an example of the reaction of immobility; and, if so, what are its functions? Can we yet explain the tendency possessed by some persons to faint at threatened pain, and at the sight or the sound of the sufferings of others? It is certain that in the second case the individual often sympathetically feels the other's pains, but why does he faint? Or again why faint at the sight of blood, and why, in some cases, perversely enough as it seems, at the sight of other people's, rather than one's own?

A new concept of dissociation is proposed, that of "a process and state in which suppressed experience acquires an independent activity," which "carries with it independent consciousness." It follows therefore that suppression often exists without anything which can be regarded as dissociation. This proposal, however, appears to raise the necessity of deciding, in any particular case, whether we are justified in assuming the presence or absence of independent consciousness, a task which unfortunately seems to imply all those very real difficulties connected with the criteria of consciousness which have driven some psychologists prematurely to the extreme left wing of behaviourism.

This difficulty reappears in the chapter dealing with the 'complex.' The conception of the complex, described by Bernard Hart in the first popular English exposition of Freud's and Jung's ideas, *The Psychology of Insanity*, is rejected as "almost identical with the 'sentiment' of the orthodox psychologist." The differences between a sentiment and a complex, according to Dr Rivers, are first, that the sentiment is *more* complex than the 'complex,' the characteristic features of which are its relation to the unconscious and its affective importance; secondly, that the sentiment is a far more epicritic product than the complex; sentiments "take part in the most finely graduated processes, and are connected with discrimination of the most delicate description."

This distinction cannot be dealt with in a sentence or even in a paragraph, for it is the view which it expresses, rather than the suggested definition, which offers itself for criticism. Dr Rivers's opinion is that "nothing but confusion can result from the inclusion in one category of definitely pathological processes and such absolutely normal and necessary processes as the sentiments." But might it not be added that it is just these practically useful distinctions between health and disease, which obscure important theoretical issues by suggesting hard divisions where none actually exist? The terms 'clean,' 'honest' and 'sober' may be indispensable when one is writing a testimonial, but they are merely challenges for thought, labels for problems, to the student of hygiene, ethics or physiology. Sane and insane, psychoneurosis and psychosis, *mens sana in corpore sano*—how do they help us except as blunt practical distinctions?

So what one man considers to be his sentiments another may term his complexes. And do not some sentiments, *e.g.* those of love for a person of the opposite sex, or for one's native land, often suggest, by their relative absence of delicate discrimination and of graduation, perhaps even by the repression which their growth necessitates, the characteristics of the 'complex'?

It is certain that many other reviewers will prefer to deal more fully with the latter part of the book: that which aims at providing a foundation for a biological theory of the psychoneuroses[1], and for this and other reasons the present essay attempts to treat the earlier chapters in detail. The view put forward is that mental health depends upon the presence of a state of equilibrium between instinctive tendencies and the forces by which they are controlled. The psychoneuroses arise as attempts, successful or unsuccessful, to restore the balance between the instinctive and the controlling forces.

> The relative simplicity of the war-neuroses is due to their origin in disturbance of the relatively simple instinct of self-preservation, while the great majority of the neuroses of civil practice depend on the failure of balance between the less simple sexual instinct and the very complex social forces by which this instinct is normally controlled.

The two great divisions of the psychoneuroses: the hysterias and the anxiety-neuroses, with their different symptoms and their characteristically different affective colourings (an important aspect, well brought out) are re-named 'Substitution Neurosis' and 'Repression Neurosis' respectively. They are explained along the lines for which the earlier writings of Dr Rivers have prepared the reader. His theory of the former seems more likely to arouse dissentients than that of the repression neurosis. It is that hysterical symptoms are modified forms of the instinct of immobility, and that hysteria results as a product of the two processes of suppression and suggestion; the latter term having been given a somewhat special meaning. But he believes that two forms of hysteria may be distinguished, differing in their aetiology; and thereby supplies a subject complex and many-sided enough for a whole international congress.

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[1] See, for example, the review by Dr T. W. Mitchell; This *Journal*, Medical Section, 1921, I. 165–173.

Features important to a complete understanding of the book, which unfortunately cannot be treated here for lack of space, are the conceptions of Suggestion, Intuition, and the treatment of hypnosis and sleep. The appendices, especially those entitled "A Case of Claustrophobia" and "Freud's Conception of the 'Censorship,'" are so full of matter that no just account which is at the same time brief, is possible.

Perhaps Dr Rivers would agree if one ventured to call this novel book the record of an impression. The present review is no more than an attempt to write down an impression of an impression.

PUBLICATIONS RECENTLY RECEIVED.

The Religious Consciousness: a psychological study. By Professor JAMES BISSET PRATT. New York: The Macmillan Company. 1920. Pp. 486. $4.00.

In many ways this is the best work that has yet appeared on the psychology of religion. The author has succeeded in describing the religious consciousness from the point of view of an unprejudiced observer who has no thesis to prove. At the same time, he has that sympathetic insight into religion which is necessary even for purely descriptive work.

The two chapters on conversion are new and extraordinarily interesting. There is a careful study of the purely emotional conversion, of which Bunyan and Brainerd are examples, which became the conventional religious crisis of some forms of protestantism. The last five chapters are on mysticism, and give an account of that difficult subject which is not unsympathetic to the mystic, while it allows due weight to the point of view of mental pathology.

Prof. Pratt has written a very valuable work which, however, does not claim to be a complete treatment of its subject. Probably the chapter on "Religion and the Subconscious" does not attach sufficient weight to the importance of the work of Freud and his followers. It is, however, a book which no student of the subject can afford to neglect.

Movement and Mental Imagery. By Professor MARGARET FLOY WASHBURN. Boston and New York: Houghton Mifflin Co. 1920. Pp. xv + 252. $1.75 net.

Acting on the belief that "in psychology especially one may be permitted to push theory ahead of fact" the author puts forward in this book the sketch of a motor theory of mental process, attempting to show how the whole of the inner life is correlated with and dependent upon bodily movement. Taking as a working hypothesis the idea that all consciousness is related to movement, that it, in fact, accompanies "a certain ratio of excitation to inhibition in a motor discharge," the essential nature of the different mental processes is reviewed in the light of this theory. So, the checking of the normal motor response to any external stimulus by the influence of some antagonistic motor response gives the conscious experience known as sensation. Attention is the actual performance of movements. Movements lie at the root of all associations..."associative dispositions consist of lowered resistances between the kinaesthetic pathway involved in the performance of one movement and the motor pathway involved in the performance of another."

But most interesting, perhaps, is the way in which the theory is used to explain the nature of images..."whenever a motor pathway is at the same time excited by a sensory pathway and partially inhibited by an antagonistic motor excitation a process occurs in all sensory pathways connected with the motor pathway by low synaptic resistances, including the sensory pathway that is exciting the motor pathway in question. This process is accompanied by consciousness....When it occurs in a sensory pathway that is being excited by an outside stimulus it gives rise to the type of consciousness that we call a peripherally excited sensation. When it occurs in a sensory pathway that has no outside excitation it gives rise to the type of consciousness that we call a centrally excited sensation or mental image."

Images, too, then, are nothing more than inhibited movements. Trains of images are successive movement systems carried on by the operation of motor responses which, though slight, are none the less actual. Differences of imagery are due to differences of attention, that is, to differences in motor response to stimuli. Kinaesthetic 'images'—marked off as peculiar in type by many investigators—are not images at all but sensations.

The laws of memory and the nature of purposive thought are also brought under review—while finally an explanation is offered of those 'imageless' processes so often claimed as the only conscious accompaniment of thought. These processes, it is suggested, are at root like all others kinaesthetic in nature, but since kinaesthetic processes need not be localised many of them are never fully cognised as sensations, but are experienced, as for example, in rapid thought, in forms to which no definite name can be applied.

The whole book is a skilful and suggestive exposition of an interesting theory as to the true nature of mental activity.

Das Problem der Gültigkeit in der Philosophie David Humes: ein kritischer Beitrag zur Geschichte der Erkenntnistheorie. Von Dr HEINRICH HASSE. München: Ernst Reinhardt. 1920. Pp. 194.

Kant's acknowledgment that it was Hume who had roused him from his dogmatic slumber, has not been altogether to Hume's advantage. Hegel started the view, adopted by most writers, that Hume's whole significance in the history of philosophy lay in this service of awakening Kant. Dr Hasse holds that Hume's philosophy has an intrinsic interest of its own, and deserves an independent study without reference to Kant. In the volume before us Dr Hasse deals fully with the problem of the validity of human knowledge, which he regards as the central theme round which all the philosophical questions discussed by Hume really revolve. Dr Hasse contends that Hume did not clearly distinguish between psychological and logical questions, constantly confusing the problem of the genesis of ideas with that of their validity. Hume's basic principle, that all ideas must be traced back to impressions, was narrow and ambiguous. Nevertheless, Hume himself unwittingly corrected some of his initial errors, and so much may be learned from his wrestlings with epistemological problems.

The Problem of the Nervous Child. By ELIDA EVANS. With an Introduction by C. G. JUNG. London: Kegan Paul. 1920. Pp. ix + 299. 12*s*. 6*d*. net.

This book, by a pupil of Dr Jung, is primarily intended for parents and teachers, and deals with many important educational problems. It contains much that is sound, more that is questionable and a little that is far-fetched. The statement that "the experienced psycho-analyst can clearly see during the narration of the first dream the exact cause of the trouble," savours somewhat of 'wild' psycho-analysis, and it certainly seems amazing to expect any ordinary man to be satisfied with the explanation given of the patient, who "all his later life had the feeling of being unloved" because he was neglected and laid aside for a few hours after birth. A whole chapter is devoted, though not convincingly, to 'muscle erotism,' and the case quoted at length is not taken far enough to enable a diagnosis of 'moral imbecility' to be excluded. Apart from the appearance of many loose sentences, such as "at the age of puberty she has literally and physically lost her boy," and some mistakes such as the interchange of the seventh and eighth commandments, the book is clearly and simply written; but the difficulty in discerning the wheat from the chaff makes it very doubtful whether it will be of any value to those for whom it is intended.

The Aesthetic Attitude. By Dr HERBERT SIDNEY LANGFELD. New York: Harcourt, Brace & Howe. 1920. Pp. xi+287.

The book contains, apart from the introductory discussion of the range and nature of Aesthetics, an attempt to describe the nature of the aesthetic appreciation from, what the author would call the "psychological aspect of Beauty." The substance falls roughly into five sections, viz. an account of the Aesthetic Attitude, Empathy, Unity and Imagination, Balance and Proportion and a concluding discussion on the Art Impulse. Several of these sections are illustrated by applications of the points discussed to the Fine Arts, exemplified by photographs from well-known works of art.

The best that can be said for the book is that it contains a more or less useful summary of work, both speculative and theoretical, done in Aesthetics sometime before the War. But its utility is very relative, for in point of fact very little use has been made of the material accumulated or published in any language but English. It is true that reference is made to two or three French authors like Guyau, none of them modern, and that some German writers like Lipps or Vischer, Lotze or K. Gross, are quoted; but of recent research little is noticed and none utilised. Modern French Aesthetics seems, as far as the author is concerned, not to exist—a feature not peculiar to this author;—so too with Italian aesthetic work, criticism and theory, with the solitary exception of a passing reference to 'Professor Croce.' More particularly in a subject like Aesthetics which deals with experiences and responses, not merely of a generally human, but of a specifically cultural character which is historically and otherwise perfectly well-known, such a limitation becomes a definite blemish. It suggests not merely an ignorance of the actual conditions which inspired a work or conditioned its appreciation, but lack of cultural balance, nowhere more harmful than in a subject such as Aesthetics.

The writer's treatment of the subject is curiously lacking in criticism. Apart from apparent misunderstandings such as that of Bullough's meaning of the term 'Distance' as "the amount of appeal" (p. 72) which leads for the next 30 pages to a continual misapplication of the conception, the prominence given to the Empathy doctrines in their various formulations suggests that the author is unaware of the criticisms directed against that theory ever since it passed its zenith of popularity after about 1905. Neither the general trend of aesthetic theory nor Croce's incisive teaching on that point—going back in Italian aesthetic criticism to the 18th century—has here prevented the reappearance of the old discussion of the relations of 'Form' and 'Content' which we had believed to be safely dead. There is no relation between the various topics he chooses to discuss in the different sections of his book.

The chapter on "Unity and Imagination" roused hopes of some really helpful contribution to the burning problem of artistic Imagination, merely to offer a disquisition of attention which belongs to general Psychology, ending in a purely functionalist theory of the worst type which differs in no way from the old eye-movement doctrine, disproved, as the author points out himself (p. 125) by Stratton's admirable experiments. Nor does the exposition of the Art Impulse add anything to that well-thrashed straw, the Play theory, or to the excellent summary of anthropological material of Y. Hirn.

The Child Vision. By DOROTHY TUDOR OWEN (Mrs D. TRUMAN). Manchester: The University Press, and London: Longmans, Green & Co. 1920. 6*s.* 6*d.* net.

The title of this book indicates far less and far more than its subject, which is a very clear exposition of a method of teaching composition in schools. The difficulty experienced by the average child, as Professor Archer expresses it in his preface, is that "when he is called upon to say something he has nothing to say," and therefore he is compelled "at one and the same time to go through two processes either of which is sufficient to tax his full powers, that of finding something to say and that of thinking how to say it." Mrs Truman's method is to take the visual images in the child's mind as the material for him to express in language which will convey his meaning to his classmates. They, in their turn, draw the picture from the description, and thus afford material for comment of two kinds: criticism by the original describer, of the various drawings, and criticism of his description by the artists. In this way the essential rules for good composition—arrangement, proportion, exactitude, vividness, detail and the like, are strikingly illustrated.

The book is well written, contains many convincing illustrations of the value of the method, and is full of suggestions which cannot fail to interest anyone, teacher or student, who is concerned with the difficult practical problem of thought-expression.

1. *A Classification of Association Categories and their Use in testing General Intelligence.* 2. *A Test for the Quick Grading of General Intelligence by Groups.* By M. DAINOW and E. R. MASON-THOMPSON. The Pelman Laboratory of Applied Psychology. N.D. Pp. 23. 5*s.* net.

In the first of these two essays, the authors classify associations under twelve classes. They then attempt to grade these classes in order of intellectual priority on the basis of their experience with 50 subjects. From these subjects they finally selected 15, who were graded in general intelligence by three persons who knew them well; and after applying a suitable weighting principle to the different classes of their associations, the authors come to the conclusion that "the association reaction type can be used as a good index of general intelligence." The test recommended in the second paper for the "quick grading of general intelligence" consists in reading aloud as rapidly as possible, so as to make sense of, a paragraph of 183 words in which the letters of each word are printed in reversed order, *e.g.* 'dna' for 'and.' The subject is given the key, if at the end of 1½ minutes he has not arrived at it, and he is then allowed to proceed. The writers claim that this test serves to divide subjects very sharply into broad grades of intelligence.

Instinct in Man. By Dr JAMES DREVER. Second edition. Cambridge: The University Press. 1921. Pp. x + 293. 10*s*. 6*d*. net.

The appearance of the second edition of this excellent book proves that its outstanding merits have been widely recognised. "The only significant difference between this and the first edition is the new appendix on 'The Emotional Phase of Affective Experience'" which "gives the gist of a paper read before a general meeting of the British Psychological Society on the 19th of June, 1920." The main object of this interesting essay is to show "that instinct feeling as such is not necessarily emotional," and that "we should speak of 'instinct feeling' where McDougall uses 'emotion' to designate the affective element in instinct experience."

NOTES ON RECENT PERIODICALS.

L'Année Psychologique (21st year) 1920.

With this volume *L'Année Psychologique* resumes publication for the first time since 1914. The first place among the original papers is occupied by a long study of *The Problem of Expectancy* (*Attente*) by Mlle Moraud, who compares the introspections of her subjects during the period intervening between the warning of an imminent signal and the perception of the signal itself, with graphical records of muscular tone and movement. She concludes that the state of expectancy is extremely complex in any given individual and varies greatly among different individuals according to whether their modes of thought are rich or poor in imagery.

M. Bourdon reports some experiments on the *Localisation of Sound*; he finds, among other results, that differences in direction are most easily detected in the case of sounds moving in a horizontal plane. The perception of 'up' and 'down' differences is particularly defective.

M. Foucault examines the *Persistence of Practice Effects* exemplified by the Kräpelin addition text. He concludes that:

(i) Capacities acquired by practice become weaker from inaction and this weakening, measured by the increased time necessary for performing the work, appears to be a function (not yet determined) of the period of inaction. The decline may be as much as 25 per cent. in a year.

(ii) The capacity thus weakened regains its former level after a very brief period of renewed practice; if the practice be continued the capacity may be further improved to an extent of some 10 or 15 per cent.

(iii) The resistance to the deleterious effects of inaction, the speed of recovery to the former level and the possibility of further improvement are dependent on the period and, probably, also on the method of practice.

M. Piéron, the Editor, contributes an account of researches on the *Memory of Forms* (consisting of groups of lines) and of tables of figures. He finds that the same laws as to the rate of forgetting, etc. hold good in each case. He attributes the effects in each case to "physiological laws affecting the formation and effacement of memory traces; traces consisting of a simple wearing of associative paths."

The same writer, in the section devoted to Notes and Reviews, gives a valuable summary of the work which was done by various nations during the war on psychological tests for aviators.

Drs Mignard and Gilles contribute an essay on the *Psychological Aspects of Psychoneuroses* based on observations at the Psychiatrical Centre of the Sixth Army.

A summary of work on this subject (mainly French) is also given by M. Wallon.

Mlle Vlalcou studies the relations existing between the span of apprehension (or 'immediate memory'—a term which she rejects), the speed of learning and the power of retention of letters, figures and nonsense syllables.

She finds a positive correlation between the first two variants, none between the first and third, and a negative correlation between the second and third. There is a positive correlation between letters and figures, between figures and syllables and, especially, between syllables and letters. She also investigates sex differences with regard to these capacities and the effects of age upon them.

Mlle Grzegorzewska contributes a paper on *Types of Aesthetic Ideation*. She discriminates the descriptive, sensory, critical, interpretative, and emotional types and finds that of these the intellectual varieties are, in general, characteristic of men, the emotional of women.

Abstracts of about 600 psychological books in French, English, Italian and German are also given.

Archives de Psychologie, Vol. XVII.

No. 67, April 1919.

Instinct, Émotion et Sentiment. (J. Larguier des Bancels.)

Part 1 considers and accepts W. James's theories of instinct. The author then goes further and considers the instinctive reactions to Fear and Anger.

Part 2 deals with Emotions in relation to instinct. Emotions are regarded as substitutes for instincts in some cases of emergency and in the case of the psychoneuroses.

Part 3 deals with Sentiments in relation to emotions. All excessive sensations are painful. Sentiments are believed to be subjective and in no way dependent on organic reactions as are emotions.

(1) *Symbolismes en Psychopathologie.*
(2) *Quelques remarques sur le Symbolisme dans l'Hystérie.* (H. Flournoy.)

(1) Examples of dreams, hallucinations, illusions of pathological patients are cited and an explanation of the symbols used given, showing how a suppressed experience comes to the surface, often modified by the censor. Cures may be effected by means of psycho-analysis and free association.

(2) An account is given of cases of Hysteria, with the methods of curing them. Suggestion is largely used. Symbolic motions are considered as arising from some kind of suggestion. Many observations are needed to ascertain what a given symbol represents. There is a general discussion on the meaning, use and interpretation of such symbols.

Réflexions sur la Classification et l'Unification des Sciences. (Ch.-Eug. Guye.)

The bearing of the theory of relativity on our present conception of Time and Length is discussed. The sciences are then classified according as they deal with the increasingly complex conception of Numbers, Space, Time, Material, Life and Thought. A discussion follows of the connecting link provided by the Theory of Relativity between the material sciences and Psychology, maintaining that this theory has supplied the missing link necessary to connect all sciences together.

No. 68, December 1919.

Des Réactions Organiques accompagnant les États Psychologiques. (Lucien Cellerier.)

The bodily reactions to sensations, both of pleasure and pain, are the same. Variations of respiration, rate and amplitude of heart-beat, blood-pressure, and the volume of the limbs and brain are considered, and extensive experimental data given. Conclusion is that the change in bodily state is due to change in affect quite independent of the nature of the affect.

A Propos d'un Disciple d'釋nternährer. (Raymond de Saussure.)

An account is given of the life and history of Unternährer and his religious sect, describing the sexual nature of his doctrines. The explanation of the fact of the existence and continuance of this sect is due to the inability of certain abnormal individuals to suppress their sexual tendencies. The study of the sect and its doctrines is of especial interest and value to the Freudian school.

Recherches sur la Corrélation entre la Mémoire des Mots et la Mémoire des Images. (Yvonne Delhorbe.)

An account of work done to see if children with a good memory for words have also a good memory for images. A correlation of ·42 was obtained, most children having the best memory for images.

(1) *Percentilage de quelques Tests d'Aptitude.*

(2) *De la Constance des Subjects à l'égard des Tests d'Aptitude.* (Ed. Claparède.)

(1) Account of four types of tests given to school children: 1. Memory of 15 words. 2. Permutations of four letters. 3. Rapidity of writing. 4. Arithmetic calculation. The value of the results is discussed showing that the median of results should be taken as representing the group most truly. The memory test shows natural aptitude, while the other three tests show intelligence. The mental age of the subject cannot confidently be estimated from these tests.

(2) Memory tests were given four and six times to find how constant various subjects were in their reactions. A definite constancy in the capacity of the individuals was observed, but the number of observations necessary for the accurate classification of a group of people is not decided, though four or six is suggested.

Brain, Vol. XLIII.

Part I, May 1920.

The Results of Secondary Suture of Peripheral Nerves. (J. S. B. Stopford.)

Microgyria and its Effects on other Parts of the Central Nervous System. (W. Page May.)

A Psychological Interpretation of Essential Epilepsy. (L. Pierce Clark.)

A study of the behaviour of potential epileptic individuals. Essential Epilepsy is dependent upon a primary congenital defect of natural adaptations to reality. Psychologically considered, the fit is a protective mechanism, reducing the subject's attachment to reality.

The Psychogalvanic Reflex: A Review. (E. Prideaux.)

An account of the observations and conflicting opinions of many observers, showing the need for further investigation.

Forms of Peripheral Neuritis among Troops serving with the Egyptian Expeditionary Force, 1915–19. (F. M. R. Walshe.)

Part II, July 1920.

Aphasia and Kindred Disorders of Speech. (Henry Head.)

An account of results based on the investigation of young men suffering from the effects of gunshot wounds in the head. Chap. I: Serial tests employed. Chap. II: Criticism of previous explanations of Aphasia. Chap. III: Functions affected in Aphasia, etc. grouped as "Symbolic thinking and expression," because the most definite disturbance is to be found in the use of words, figures and other symbols. Chap. IV: Various dissociated forms comprised under four heads: (*a*) Verbal defects; (*b*) Nominal defects; (*c*) Syntactical defects; (*d*) Semantic defects. Chap. V: "Symbolic thinking and expression." A full summary of this investigation has been given by Dr Head in this *Journal*, 1921, XI, 179–193.

Congenital Familial Spinal Muscular Atrophies and their Relation to Amyotonia Congenita. (Knud H. Krabbe.)

Part III, November 1920.

On the Symptom-Complexes of Lethargic Encephalitis with Special Reference to Involuntary Muscular Contractions. (F. M. R. Walshe.)

On Decerebrate Rigidity in Man and the Occurrence of Tonic Fits. (S. A. Kinnier Wilson.)

Relation of the Form of the Knee-Jerk and Patellar Clonus to Muscle Tonus. (Henry Viets.)

Alterations of the Golgi Apparatus in Nerve Cells. (Wilder G. Penfield.)

Journal of Educational Psychology, Vol. XI.

No. 1, January 1920.

The Translation Method of Teaching Latin. (W. H. Fletcher.)

Translation of Latin into English constitutes the greater part of each exercise. Everything new is taught in its functional relation within a sentence composed of material otherwise known. An outline of a typical course of lessons is given.

The Psychology of Riddle Solution: An Experiment in Purposive Thinking. (Thomas Russell Garth.)

Riddle-solution shows the use of the method of trial and error in the solution of problems. The best results were obtained by those who showed neither paucity nor redundancy of suggestions.

An Inquiry into the Statistical Basis of a Conclusion Concerning Sex Differences. (George W. Frasier.)

The conclusion put forward by Yerkes, Bridges and Hardwick in *A Point Scale for Measuring Mental Ability* is criticized on the ground that it is based on the modal value, which varies with the size and position of the class-interval.

Communications and Discussions: Age-Grade Distribution and Intelligence Quotient. (Allen J. Williams.)

The increase of 'overageness' up to the fourth grade is explained by the fact that 2 per cent. of all children cannot do more advanced work. The over-age group should be balanced by an under-age group.

No. 2, February 1920.

The Administration of the Psychology Prerequisite to Courses in Education. (Clifford Woody.)

Data gained by a questionnaire show that the current practice is for courses in education and psychology to be controlled in independent departments. The department of education usually determines its own prerequisites. The need for the definite standardisation of the prerequisites is emphasised.

The Use of Tests in the Evaluation of Methods of Instruction. (Buford Johnson.)

The results of the application of standardised tests to a number of children are given. The most effective use of the tests is the determination of methods of instruction to meet individual needs.

A Brief Group Scale of Intelligence for Use in School Surveys. (Sidney L. Pressey.)

A short scale consisting of four tests dealing with verbal ingenuity, logical judgment, arithmetical ingenuity, and moral judgment, is described. It can be used both for comparison between schools and for individual diagnosis.

The Use of Psychological and Trade Tests in a Scheme for the Vocational Training of Disabled Men. (Ralph S. Roberts.)

Vocational training must be based on psychology. Knowledge of the man to be trained must be gained by intelligence tests, trade tests and experts interviewing. Cooperation with manufacturers and trade unions is necessary.

Communications and Discussions: Some Results of Monroe's Diagnostic Tests in Arithmetic. (Julie E. Wertheimer.)

The results of seven tests showed weak work in the fundamental operations. The results were made the basis of improvements in teaching methods and a re-application of the tests showed an improvement of 100 per cent.

No. 3, March 1920.

Measurement of Ability in Latin. Part III. Vocabulary and Sentence Tests. (V. A. C. Henmon.)

In two previous papers five vocabulary tests and three sentence tests were put forward. In this paper scale values for eighty additional words and 60 additional sentences are given.

The Rational Learning Test Applied to Eighty-One College Students. (Joseph Peterson.)

The test attempts to measure quantitatively the effectiveness of a continued reasoning process. The results of the application of the test to college students are given and correlated with the standing of the students in psychology. Decrease of errors in the test seemed to mean increase in scores in psychology, but the converse was not so generally true.

The Effect of Practice on Two Mental Tests. (J. F. Dashiell.)

The two tests are the 'mental formation' test and the 'free association' test. The former shows no improvement with practice: the latter shows definite effects of practice in the case of boys, while for adults the results are equivocal.

The Adolescent Will-Profile. (June E. Downey.)

A series of tests designed to throw light on a number of character traits is described. The adolescent tends to excel in the tests revealing force of reaction and in those showing ease and fluency of reaction, and to obtain a low score in those requiring precision and carefulness of reaction. Illustrations of individual differences in will-profile are given.

Communications and Discussions: Classification of Junior High School Pupils by the Otis Scale. (W. D. Armentrout.)

An account of the Otis classification of the pupils of a particular school.

Correlation of General Intelligence Tests and School Standing. (E. B. Skaggs.)

A very low correlation was found. The need for a complete summary of previous work on this subject is emphasized.

No. 4, April 1920.

Mental Tests as a Means of Selecting and Classifying College Students. (Agnes L. Rogers.)

The correlation between the standing in the tests and the academic standing was small, but the tests might form a useful supplementation of existing methods of selecting students if the period of testing were lengthened, and more fundamental capacities measured.

Students' Methods of Studying a Certain Subject—Psychology. (L. W. Webb.)

A questionnaire showed that the general tendency of most students is to use poor methods of study. Good students were found to possess better study-habits than poor students. The need for instruction in methods of work is emphasized.

An Educational Survey Test. (Rudolf Pintner and Florence Fitzgerald.)

A short Educational Survey Test is given in detail. This test shows a good correlation with the results of the long scales from which it is derived and represents an economy in time, labour and cost.

Results of the Bell Chemistry Test. (Thomas H. Briggs.)

The test was given to 38 boys and the results show that while the test is a fairly good measure of a class knowledge of chemistry it is unreliable as a measure of the individual. A correction sheet for the test is given.

No. 5, May–June 1920.

Graded Opposites and Analogies Tests. (M. J. Van Wagenen.)

An account is given of the construction of a series of analogies tests and a series of opposites tests of approximately equal difficulty. The tests with their adequate responses are given in detail.

A Study of Twenty High School Seniors of Superior Intelligence. (Dorothy Hazeltine Yates.)

A study of twenty pupils selected by means of intelligence tests showed that the superior pupils came from homes suitable to development and were generally precocious physically as well as mentally. Their interests were more intellectual and their health not below normal.

A Test of Some Standard Tests. (Grace E. Bird.)

The correlations found to obtain between certain standard tests and general ability are given. The tests involving the most language ability were the most reliable indices of general school ability while those which were the least interesting in form to the pupil were of least diagnostic value. The tests need to be more easily administered and scored.

The Reliability and Significance of Tests of Intelligence. (Edward L. Thorndike.)

An examination of twenty to fifty minutes is adequate to make only coarse distractions among individuals. Intelligence as measured by verbal tests is very different from intelligence as measured by non-verbal tests.

Psychological Monographs, Vol. XXVIII.

No. 123. *Quantitative Aspects of the Evolution of Concepts.* (Clark L. Hull.)

Learning of "isolated concepts" gave as good results as the evolution of concepts from concrete instances. The evolution is only about one-sixth as rapid for abnormal as for normal subjects. Moderate familiarity with a large number of cases is more economical than complete familiarity with half as many cases. Trial and error plays an important part in the evolution of concepts.

No. 124. *An Experimental Analysis of a Case of Trial and Error Learning in the Human Subject.* (George S. Snoddy.)

A detailed account of the process of learning mirror-tracing which is shown to be of the true trial-and-error type. In the early stages of the learning the period of rest is the important factor, and attention to accuracy gives the best results. In the later stages continuous practice is more efficient and secures progress in both speed and accuracy. The pleasure following a successful movement has no place in bringing about improvement. A physiological account of the rest period, based on the conception of the Multiple Final Path, is put forward.

No. 125. *Work with Knowledge of Results versus Work without Knowledge of Results.* (George F. Arps.)

Both the absolute amount of ergographic work, and the speed of work, were found to be greater when the subject was allowed to see the record during the work. In the later experiments concrete imagery of results appears in the 'unknown' series and takes the place of the perception of results in the 'known' series, and the difference between the results diminishes.

No. 126. *Individual Differences in Finger Reactions.* (Esther L. Gatewood.)

The speed and accuracy of the reactions of the fingers both singly and in combination are investigated. The fingers of the right hand react more quickly and more accurately than those of the left. Two-finger reactions are faster and more accurate than one-finger reactions. The most efficient combinations are those involving one finger of each hand. Practice increases the speed and accuracy of all fingers and tends to reduce the differences between them.

No. 127. *The Lag of Visual Sensation in its Relation to Wave-Length and Intensity of Light.* (Marion A. Bills.)

Four older methods of investigating the order of speed with which visual sensations reach their maximum are compared with three new methods. The new methods aim also at a comparison of the effects produced at different times before the maximum is reached. Close agreement was found between all the new methods and two of the old as regards the time required to reach the maximum, and between the new methods as regards the rate of the rise. The rate of rise differs for different wave-lengths and the order of the rate of rise varies with the intensities of the lights.

Journal of Experimental Psychology, Vol. III.

No. 1, February 1920.

Conditioned Emotional Reactions. (J. B. Watson and R. Rayner.)

Conditioned emotional reactions, once established, can, when the subject is in infancy, be transferred to other objects which have some characteristics in common;

further, they persist all through life, and emotional disturbances may be traced in adults to responses set up in infancy. All three of the fundamental human emotions, fear, rage and love, cause such responses, consequently Freud is wrong when he traces them only to love or sex.

The Organic Effects of Repeated Bodily Reaction. (C. R. Griffith.)

The organic effects of repeated bodily rotation are highly variable in their appearance and so amenable to practice that they may entirely disappear within a relatively short time, provided rotation is repeated from day to day.

A Series of Form Boards. (G. O. Ferguson, Jr.)

A Description of an Apparatus for Determining Acuity at Low Illuminations. (Ferree and Rand.)

For testing light and colour sense and for detecting small errors in refraction and in their correction.

Reaction-Time Symptoms of Deception. (W. M. Marsten.)

Two distinct types of reaction-time behaviour during deception:

(1) *Positive type* characterized by increased length of reaction-times and increase of mean variations.

(2) *Negative type* giving shortened reaction-times which appeared to have no consistent effect upon mean variation.

A third group of subjects shifted from positive to negative types on different days.

No. 2, April 1920.

An Experimental Study of Dizziness. (C. R. Griffith.)

The organic and mental effects of dizziness are readily affected, in their appearance and duration, by the direction of attention, psycho-physical determination, the character of the visual field, and the mode of ocular fixation.

Interdependence of Judgments within the Series for the Method of Constant Stimuli. (S. W. Fernberger.)

A contrast effect is present between the pairs of stimuli under the conditions specified. This causes a distortion of the curves for the psycho-metric functions for the 'lighter' and 'heavier' judgments, and increases with practice.

Discussion: Note on Mr G. J. Rich's Study of Tonal Attributes. (Henry J. Watt.)

Reply to Dr Watt's 'Note.' (Gilbert J. Rich.)

No. 3, June 1920.

The Influence of the Group upon Association and Thought. (F. H. Allport.)

The presence of a co-working group is favourable to the speed of Free Association. The group produces more ideas of thought but these are of a lower logical value than those produced in solitude.

Attributive v. Cognitive Clearness. (Carl M. Dallenbach.)

Britz's perceptual steps are verified, and extended. The end result of experiments on 'range of attention' is that they involve not 'attention' but 'cognition.'

Curves of Growth of Intelligence. (H. A. Toops and R. Pintner.)

These curves are based upon data derived from a specific set of tests, and though too much increase may be shown in the higher ages, they will serve to indicate the general trend of mental development for all degrees of intelligence at the various chronological ages.

No. 4, August 1920.

The Use of the Illumination Scale for the Detection of Small Errors in Refraction and in their Correction. (C. E. Ferree and Miss Rand.)

This method would seem to have its chief value in determining the exact amount of the correction in cases of high astigmatism, and both the amount and placement of correction in cases of low astigmatism.

The Backward Elimination of Errors in Mental Maze Learning. (Joseph Peterson.)

Two methods of studying maze-learning emerge from this paper: (1) an empirical method which enables the problem to be studied independently of spatial factors peculiar to individual mazes, and (2) a method of comparing the distribution of errors in the maze with the expected distribution based on chance.

Some Volitional Patterns Revealed by the Will-Profile. (Miss Downey.)

The outcome of this investigation indicates that the 'Will-Profile' can be used in determining the specific combination of traits of character of an individual in addition to his general temperamental pattern.

On the "*After-Sensation*" *of Pressure* (R. T. Holland) the following conclusions are arrived at:

A. If the observer is passive towards the experiment after-experiences are few and brief.

B. If he assumes an active attitude, they are numerous and of considerable duration.

C. When true after-sensations of pressure *do* occur, there is also no doubt that pressure-imagery plays a part in the formation of cutaneous after-experiences.

Psychological Bulletin, Vol. XVII.

No. 1, January 1920.

In analysing "the operation of the laws of chance in tests of the two-alternative type," S. C. Kohs finds that "opinion in the past has been inclined to exaggerate the frequency of zero scores, and to under-estimate or overlook the frequency with which significant positive scores might occur."

The following are responsible for reviewing 1918–20 publications under the accompanying subject-groups: (1) W. T. Mason, "Mind and Body,"—the Behaviourist and Dualist standpoints. (2) A. P. Weiss, "Consciousness and the Unconscious." (3) H. S. Langfield, "Textbooks and General Treatises."

C. E. Longshore describes new apparatus invented. H. C. Warren notes movements towards standardisation of terminology.

No. 2, February 1920.

This number reports the 28th Annual Meeting of the American Psychological Association, with abstracts of the fifty-seven papers read.

No. 3, March 1920.

Floyd Allport surveys the publications of 1918–20 in the realm of Social Psychology under three groups, viz.: (1) "social-mind" theories, (2) the social behaviour of the individual, and (3) social behaviour in relation to society. He considers that "in spite of the persistent speculative essays of the old school, the trend is encouragingly towards observation and experiment."

George A. Coe summarises works which have appeared over the same period on the Psychology of Religion, while the more prominent of these are reviewed by J. H. Leuba.

Under the heading "Crime and Social Psychology," Annie L. Kellog outlines some twenty recent articles on crime "which lay the emphasis very largely upon psychiatric problems."

No. 4, April 1920.

In reviewing the work of the past three years on "Cutaneous and Kinaesthetic Senses," John T. Metcalf draws special attention to Lashley's article on "The accuracy of movement in the absence of excitation from the moving organ."

In his survey entitled "Affective Phenomena, Descriptive and Theoretical," H. N. Gardiner outlines recent contributions to the study of the emotions.

Special reviews are devoted to Cannon's "Physiological Basis of Thirst" and Goddard's "Psychology of the Normal and Subnormal."

Controversial discussion is entered into between Ferree and Rand, authors of a paper on "The selectiveness of the achromatic response of the eye to wave-length," and their reviewer Dr L. T. Troland.

The Committee on Terminology specifies certain lines on which it invites the cooperation of psychologists.

No. 5, May 1920. Physiology number.

Edwin B. Holt gives a comprehensive survey of recent experimental work on the Nervous and Muscular Systems. He groups his subjects under the topics of (1) physiology of the autonomic nervous system, and (2) the working of the twofold neuromuscular process.

F. W. Carpenter deals with some further papers falling under the first of these subject-groups such as: the effects of different kinds of emotional excitement on heart-beat and blood-pressure, on catalase content and the accumulation of hyperglycemia, of the anatomical and histological structure of the autonomic nervous system, of the gastric hunger-mechanism in fasting and fever, and of autonomic functions and the personality.

No. 6, June 1920. Comparative Psychology number.

Wallace Craig reviews the results of recent research into "Tropisms and Instinctive Activities."

In surveying the previous year's work on "The Sensory Physiology of Animals," K. S. Lashley finds that "the bulk of material deals with photic sensitivity" while "labyrinthine functions occupy second place."

John F. Shephard summarises recent papers on "Habit Formation and Higher Mental Capacities in Animals."

Special reviews are devoted to Ritter's "Unity of the Organism," and G. V. Anrep's "Pitch Discrimination in the Dog."

The Training School Bulletin, Vol. XVII.

Nos. 1 and 2, March–April 1920.

S. D. Porteus and Helen F. Hill outline a condensed guide to the Binet-Simon tests.

Part I. Tests and Diagnosis.

A just estimate of the value and limitations of these tests is now being approached. The importance of supplementary tests to counter-balance the 'over-literary-emphasis' in Binet verdicts is urged. Goddard and Stanford revisions are considered and recommendations made as regards: (1) percentile tabulation of results for statistical research purposes, (2) scale modifications for infants and adolescents, (3) modifications in diagnostic methods.

Part II. Tests and Test-Procedure.

A table is drawn up, supplementary to Terman's "Record Blank," with clear directions for giving each test, together with descriptions of material required and conditions of success; explanatory notes and commentary are appended.

No. 3, May 1920.

J. E. Wallin dealing with "The Concept of the Feeble-Minded" concludes that:

(1) Congenital (or early acquired) intellectual deficiency resulting in social and economic incompetency is the sole criterion of feeble-mindedness.

(2) The term 'moron' should be reserved for those persons whose intelligence level at maturity is below 9 years: even then its application is not universal.

(3) Those persons, whose intelligence level is above 9 years requiring custodial care on account of conditional or volitional instability, may be termed 'defective delinquents,' and should be accommodated apart from the feeble-minded.

No. 5, September 1920.

In Part I of a paper on "Congenital Word-Blindness," J. E. Wallin defines this defect as "inability to understand or interpret printed or written characteristics." Its incidence is probably much more general than hitherto supposed, and it appears to be the primary reason for some children's slow school-progress. The defect has been more often found in boys than girls, and its physiological basis is considered. It is due to the interpretation of defective visual word-imagery, not to defective apprehension or retention of ideas once grasped.

The work of the Vineland Research Department is outlined; problems within its general scope are: (1) the recognition, (2) the causation, and (3) the prevention of mental defect.

No. 6, October 1920.

Concluding his paper on "Congenital Word-Blindness" Wallin proves that "word-blind children are not deficient in visual and auditory imagery." The extent of the defect varies in individual degree, not in kind. Special classes are advocated, with more oral instruction and special reading-lessons.

E. R. Johnstone writing on "The management of an Institution for Mentally Defective Children," explains methods of enlisting the confidence and co-operation of parents.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

February 5, 1921. Reaction of the Will to Objective and Subjective Stimuli, by E. Boyd-Barrett.

The Functioning of the Will: a suggested application of Sherrington's work on the Reflex, by C. A. Claremont.

March 12, 1921. The Intellectual Respectability of Muscular Skill, by T. H. Pear.

A Method for the Study of Fluctuations in Mental Output: Types of Oscillations Found, by S. J. Philpott.

SECTIONAL MEETINGS.

(a) Education Section.

January 5, 1921. Mental Training through Language Studies, by C. W. Valentine.

February 8, 1921. The Upper Limit to the Growth of Intelligence, by P. B. Ballard.

March 22, 1921. Psycho-analysis and the Educator, by Barbara Low.

(b) Industrial Section.

March 19, 1921. Vocational Tests, by Frank Watts.

The Dotting Machine as a Fatigue Test in a Factory, by May Smith.

(c) Medical Section.

December 22, 1920. Psychology and the Unconscious, by T. W. Mitchell.

January 26, 1921. The Problem of the Neurasthenic Pensioner, by Millais Culpin.

March 23, 1921. 'Mary Rose' and the Problem of Infantile Personality, by Constance Long.

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